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THE
LONDON, EDINBURGH, AND DUBLIN
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CONDUCTED BY

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster
vilior quia ex alienis libamus ut apes. JUST. LIPS. *Polit. lib. i. cap. 1.* Not.

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“Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phæbus ferrugine condant,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cælo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Manzonium.



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THE
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JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1849.

- I. *On the Inorganic Constituents of Organic Bodies.* By
H. ROSE, *Professor of Chemistry in the University of Berlin**.

THE inorganic constituents of vegetable and animal substances have received more attention during the last few years than was formerly the case; and in consequence of Liebig's exertions especially, numerous investigations of the ashes of organic substances have been made; but their principal object was only technical. It was soon perceived, that as plants derive the inorganic constituents, without which they cannot exist, from the soil, it was of the greatest importance to determine these constituents with accuracy, so as to be enabled to judge whether the soil was capable of yielding them; and if not so, to allow of their being added in the form of a suitable manure.

Hitherto almost all these investigations have been directed to the determination of the relative proportions of the inorganic constituents of the ash. Occasionally, indeed, the presence of certain inorganic constituents, especially salts, has been detected by microscopic examinations; but scarcely any one has expressly taken up the question which is somewhat closely connected with this point, viz. in what manner are the inorganic substances combined with the organic? Whether they form with each other the same kinds of combinations as those which are artificially prepared in our laboratories, or whether peculiar compounds, existing only in living organic bodies, are formed by the mutual influence of the inorganic upon the organic matters, are questions which must be of great importance, especially in animal and vegetable physiology, and which have not hitherto been accurately investigated.

In very few instances only have researches of this kind been attempted. Endeavours have been made, for instance,

* From Poggendorff's *Annalen*, vol. lxxvi. p. 305.

to show the manner in which the iron is combined with the other constituents of the blood. But these and some other experiments of the same kind form isolated examples ; at least it has never been attempted to ascertain the changes which the inorganic constituents supplied to vegetable and animal bodies by the soil and articles of food undergo within them.

Some time since, I endeavoured to prove, that when an organic substance of vegetable or animal origin is carbonized with exclusion of the air, by not too great a heat, the inorganic constituents can partly be extracted by the ordinary solvents of inorganic salts—water and muriatic acid, but that a portion, and this frequently the largest, exists in the carbonized residue of some organic substances in such a state as completely to resist the solvent action of water and muriatic acid, and can only be obtained by burning the carbonized mass in oxygen gas or in atmospheric air*. This portion of the inorganic matters undoubtedly does not exist in the organic substance in the state in which it is obtained after the incineration, nor even in the carbonized mass obtained from it, but it has been formed by oxidation. I also described several experiments, which showed that the presence and the nature of the carbonized mass could not be the causes of the insolubility of the inorganic substances in the solvents, even if they pre-existed in the organic bodies in the same state as that in which we find them in the ash.

These remarks, which I published in an imperfect state and supported by few proofs, for the sake of directing the attention of chemists to this, as it appeared to me, not unimportant subject, have not received any attention. Since that time, I have, however, occupied myself with these investigations, and have induced several young chemists in my laboratory to determine the inorganic constituents, with a view to separating those which exist already formed in organic bodies from those which must be contained in them in an unoxidized or less oxidized state.

These investigations have completely confirmed the view I had arrived at from reflecting upon this subject. In fact I may assert, that in none of my chemical investigations has experiment so completely confirmed the hypotheses which I had made before the commencement of the undertaking than in these.

When we minutely trace the entire process by which plants and animals assimilate the inorganic substances consumed, it appears that this is effected in the two cases in an entirely opposite manner. I shall now investigate this more minutely.

* Chem. Gaz., vol. v. p. 158.

The Inorganic Constituents of Plants.

Plants obtain their inorganic constituents by means of the roots, which derive them from the soil upon which they grow. The latter either already contains them among its constituents, or they are added to it in a suitable manure. In both cases these inorganic constituents exist in the highest possible state of oxidation. If they are not contained in the manure in this state, the latter is not in a perfectly proper condition, and does not become so until it has been exposed to the air for some time.

It may certainly be admitted, that the inorganic constituents, absorbed by the root in the most perfect state of solution possible, ascend in the vessels of the stem. Probably those salts which are insoluble in water are dissolved by the aid of carbonic acid.

During the growth of the plant a deoxidizing process goes on within it; the green parts under the influence of solar light, as is well known, evolve oxygen gas. Even when they only decompose the carbonic acid of the air, they assimilate its carbon; hence the amount of the latter in the plant gradually increases in proportion to that of the oxygen. All those parts of the plant which are in contact with the green parts take a share in this deoxidizing process, so long as it continues in a state of growth and the green parts have not lost their green colour. Now when we find that in plants one portion of the inorganic constituents absorbed by the root exists in a deoxidized state, in which, at least after the carbonization of the plant, it is insoluble in the ordinary solvents—water and muriatic acid, and that this portion is not converted into the same salts as those absorbed by the root from the soil until after oxidation, it may be supposed that the quantity of deoxidized inorganic constituents must be small in those parts of the plant which are in more immediate contact with the soil, and in which, therefore, the deoxidation of the parts of the plant, and also that of the inorganic salts existing in them, first commenced. But this quantity must be greater in those parts of the plant the formation of which has required the longest time; and after the formation of which, many plants, including all the annuals, die. Hence the proportion of the inorganic constituents of the plants which are deoxidized to that of those which are not deoxidized, must be very different in the herbaceous portions of the plant and the seeds.

This supposition has been most completely verified by experiment.

M. Weber made a comparative experiment upon the inor-

ganic constituents of peas and pea-straw, which is more fully described in the appended supplement (I. and II.). They were both carbonized, with exclusion of the air, at such a temperature, that the water with which the carbonized mass was treated was not coloured yellow or brown, but remained perfectly colourless. Water extracted from the carbonized mass of the peas a large amount of chloride of potassium and phosphate of potash, some chloride of sodium and sulphate of potash, and a considerable quantity of carbonate of potash: the latter must have been contained in the peas, before carbonization, in the form of the potash-salt of an organic acid. Water extracted from the carbonized mass of the pea-straw a far larger amount of carbonate of potash, but smaller quantities of chloride of potassium, chloride of sodium, sulphate of potash, silicate of potash, and sulphate of lime.

When the carbonized mass, which had been exhausted with water, was then treated with muriatic acid, the latter dissolved out of the carbonized peas phosphate of soda, potash, lime, magnesia, and perphosphate of iron, but no earthy carbonates; whilst from the carbonized pea-straw this acid extracted a large quantity of carbonate of lime, a small quantity of carbonate of magnesia, and the phosphates of lime, magnesia and iron, but none of potash or soda.

The carbonized mass which had been exhausted with water and muriatic acid was burnt with access of air. The ash of the peas consisted of the phosphates of potash, lime, magnesia and iron; that of the pea-straw was composed of the phosphates of magnesia, lime and iron; it contained no potash, but a considerable quantity of silica.

On comparing the amounts of inorganic matters obtained from the peas and the pea-straw by these two operations, the following results are obtained. 100 grms. of peas and 100 grms. of pea-straw yielded—

	Peas.	Pea-straw.
Exhausted by water	0·380	1·417 grm.
Exhausted by muriatic acid	0·356	3·458 grms.
On the incineration of the carbonized mass	0·909	0·375 grm.

Hence water and muriatic acid dissolved very considerable quantities of inorganic constituents from the carbonized pea-straw, but not more than a sixth or seventh of this amount from the peas; whilst on the incineration of the carbonized mass which had been exhausted by solvents, far more fixed salts were formed in the case of the peas than in that of the pea-straw.

The proportion in the latter case is, however, far more

remarkable than is at first sight apparent; for the ash of the exhausted carbonized mass of the pea-straw contains more than half its weight of silica, which existed as such in the oxidized state in the living plant, and could not be obtained until after the incineration of the carbonized mass, merely in consequence of its insolubility in the solvents. If we take this circumstance into account, the exhausted carbonized mass of the peas yields five times as large an amount of fixed salts as that of the pea-straw.

Exactly the same results were obtained in the analysis of the inorganic constituents of rape and rape-straw, made by M. Weber (Appendix III. and IV.).

The quantities per cent. of inorganic constituents were—

	Rape.	Rape-straw.
Extracted by water	0·230	1·556
Extracted by muriatic acid	0·884	1·805
On the incineration of the carbonized mass }	1·364	0·570

Here also the proportion is more striking than appears at first sight, because nearly half of the ash of the carbonized mass of the rape-straw after exhaustion with the solvents, consists of silica, as in the case of the pea-straw.

The salts obtained by the incineration of the carbonized mass after exhaustion by the solvents, could not have existed as such in the plants and seeds, as has been already mentioned, nor even in the carbonized mass itself, but must have been produced by oxidation. The next question then is, in what states of combination did the unoxidized inorganic constituents exist in the plant, and what in the carbonized mass obtained from it? Upon this point all our explanations are insufficient; and this question can only be answered by hypothesis, which cannot be verified without further investigations. But at all events the subject forms a wide field for interesting investigations, which lead to important results, and by which both chemistry and physiology may acquire important elucidation.

The carbonized residue of peas and rape, after exhaustion with water and muriatic acid, yields on incineration a large amount of phosphates, and which, had they existed as such in the carbonized vegetable matter, would have been dissolved by the water and muriatic acid. The carbonized mass also contains considerable quantities of nitrogen. If the phosphorus existed in the unoxidized state, it is most probable that it formed compound radicals with carbon and nitrogen, similar to cyanogen or sulphocyanogen, which were combined with the

metals of the basic oxides contained in the ash. Hence in proportion as the deoxidizing process proceeds in the living plant, the phosphates which are absorbed from the soil by the roots become converted into non-oxidized compounds, consisting of compound radicals containing phosphorus and the metals of the alkalies and earth. These must exist in the largest quantity in those parts of the plants which are composed of substances exposed for the greatest length of time to the deoxidizing process; and these are evidently the seeds of the plants, which are formed latest, and upon the production of which the life of many plants entirely ceases.

Thus the phosphates are decomposed in plants by the deoxidizing process in the same manner as the sulphates are converted into sulphurets by deoxidation. Probably the more appropriate explanation is, that when substances containing nitrogen and carbon are present, sulphocyanurets, or compounds of a radical consisting of three elements, sulphur, carbon and nitrogen, may be formed from the sulphates by a deoxidizing process.

If we admit the occurrence of these compounds of hypothetical radicals with metals in the seeds of plants, the next point, after the determination of their properties, would be the investigation of their relation to the great mass of the organic matter in those parts of the plants in which they exist. But as even the existence of these compounds is problematical, it would be useless to form further hypotheses on this point before making ourselves somewhat more intimately acquainted with these compounds themselves.

The so-called proteine compounds appear to be formed principally in plants by the process of deoxidation; and it is precisely these substances which appear to combine with the radicals containing phosphorus, in combination with metals.

Another question must be proposed here, although it cannot be satisfactorily answered. Supposing that these compounds really exist in certain parts of plants, what changes do they undergo when the plant is carbonized with exclusion of air; when consequently all the organic matter is destroyed, and the connexion in which they probably stood to these compounds is dissolved? So long as we have no certain knowledge of the existence of these compounds, we cannot judge with certainty of the changes which they experience at an elevated temperature. It is however possible, at least not improbable, that they may be acted upon at an elevated but not too high a temperature in the same manner as the cyanurets, when they are transformed by heat into paracyanurets. Be this as it may, the experiments show that these compounds

after carbonization are insoluble both in water and in muriatic acid. But this insolubility in the solvents did not always exist before carbonization; for many organic substances, which in an undecomposed and not carbonized state are soluble in water, frequently contain a large part of their inorganic constituents in an unoxidized state.

It is well known, that most of, but not all, those inorganic salts which are insoluble in water are soluble in muriatic acid. But those salts which are insoluble in this acid certainly very rarely occur in vegetable and animal substances. The metaphosphates require most attention in this respect, since they may exist in the carbonized substance; and yet, on account of their insolubility in water and muriatic acid, their presence may only be detected after the destruction of the carbonized mass. But it is evident, from the investigations which have been made upon the inorganic constituents of vegetable and animal substances, and which are described in the Appendix, that these metaphosphates can rarely if ever be present, since in most cases carbonate of potash may be removed from the carbonized mass by water, and which, as is well known, cannot exist simultaneously with metaphosphates at an elevated temperature.

We must next mention the remarkable occurrence of silica in vegetables, principally in the stems of the Grasses and *Equisetaceæ* (Appendix V.). The silica is undoubtedly removed from the soil by the plants in the form of silicate. It becomes separated, however, from this silicate; and the silica separated forms the principal mass of the stem in several species of *Equisetum* and the Grasses. Of course it exists in them in the form of perfectly oxidized silicic acid; but in consequence of its insolubility in water and muriatic acid, the greater part of it is found in the mass remaining after the exhaustion of the carbonized substance with water and acid.

Hence on comparing the quantities of the inorganic substances which the seeds and the culm of grasses yield in the aqueous and muriatic solutions of the carbonized substance, and on the combustion of the latter, we find, in contradiction to the above determination, that the carbonized straw yields far more fixed substances than the seeds. But this contradiction is only apparent, because the substances obtained after the incineration of the carbonized straw consist almost entirely of silicic acid.

A comparative examination of the inorganic constituents of wheat and wheat-straw made by M. Weber, and which is fully described in the Appendix (VI. and VII.), proves this beyond a doubt.

Water extracted chloride of sodium and phosphate of potash and soda from the carbonized grains of wheat, but no carbonates; whilst from the carbonized wheat-straw it removed chloride of potassium, chloride of sodium, some sulphate of potash, and no phosphates, but a remarkably large amount of silica. Muriatic acid subsequently dissolved only compounds of phosphoric acid with potash, soda, lime, magnesia and peroxide of iron. From the carbonized straw, muriatic acid extracted combinations of phosphoric acid with lime, magnesia and peroxide of iron, as also some silica.

The carbonized grains of wheat, after exhaustion by the solvents, yielded on combustion, compounds of phosphoric acid with potash, lime, magnesia and peroxide of iron; also some silica. The solids remaining after the combustion of the carbonized wheat-straw consisted almost entirely of silica, with extremely small quantities of the phosphates of lime, magnesia and peroxide of iron.

On comparing the amounts of the constituents obtained in the different operations, we obtain the following results. 100 grms. of the grains of wheat and of the straw yielded—

	Grains of wheat.		Wheat-straw.	
In the aqueous solution . . .	0.471	grm.	1.216	grm.
In the muriatic solution . . .	0.562	...	0.474	...
On the incineration of the car- bonized mass }	0.246	...	2.135	...

Of the 2.135 grms. of fixed constituents which the carbonized wheat-straw yielded, 2.022 grms. consisted of silica, and only 0.113 grm. of phosphates. When this is taken into consideration, this investigation also proves that the quantity of so-called deoxidized inorganic constituents in the grains of wheat is greater than that in the wheat-straw.

Perhaps it is of importance to distinguish the organic substances, the inorganic constituents of which are contained in them in a completely oxidized or deoxidized state, by separate names.

I shall therefore call those organic substances, the inorganic constituents of which are in a perfectly oxidized state, *teleoxidic* bodies. Pea-straw and rape-straw, as also even wheat-straw, may be called teleoxidic substances, although they do not deserve this appellation so strictly as many animal bodies, of which we shall speak presently. But those organic substances, the inorganic constituents of which exist partly in an oxidized and partly in an unoxidized state, I shall call *meroxidic* sub-

stances. Peas, rape and wheat, are therefore meroxidic substances. I have never met with perfectly *anoxidic substances*, either in the examination of vegetable or animal substances. It is probable that if the proteine compounds existing in meroxidic substances were isolated as completely as possible, they would constitute perfectly *anoxidic substances*, which after carbonization would not yield any soluble salts to the solvents, until these had been produced by complete incineration.

Inorganic Constituents of Animals.

Animals derive the inorganic constituents which the various parts of their body contain, from the food. This is, however, assimilated by them in a totally different manner to that in plants; whilst in the latter, in general, a process of deoxidation occurs, to which the inorganic substances derived from the soil are subjected, in animals the nutritive substances undergo oxidation by the oxygen inspired. They are first converted into blood, and this is conveyed to all parts of the body, where reparation must occur. By the oxidation of the nutritive substances, or rather of the matters produced from them, the elevated temperature of the animal body is produced; and as this is tolerably uniform, the oxidation also must go on equally uniform in the various parts of the body. It is not, however, merely those parts of the body which consist of carbon, hydrogen, nitrogen and oxygen only, that take part in the oxidation, but undoubtedly also those compounds of the hypothetical phosphuretted radicals with metals, which such animals as are not carnivorous derive from the meroxidic substances of vegetable nutritive matters. That portion of them which is not applied to the repair of the body becomes oxidized; the same also occurs with those parts of the body which are repaired. Whilst the carbonic acid of these compounds is expired in the form of carbonic acid, and the nitrogen is converted into ammonia, the phosphorus is oxidized to form phosphoric acid, and the metals combined with the radicals, so as to form oxides. The longer these substances have been exposed to oxidation, the more perfectly are phosphates of the metallic oxides formed.

It must follow from this conclusion, that the matter first formed by the nutritive substances, the blood, from which the other parts of the body are repaired, may contain completely oxidized salts, since it is generated from meroxidic substances, but must still contain a large amount of the combinations of the hypothetical radicals with metals. Somewhat the same must occur with flesh, the composition of which is the same

as that of the blood; but as it is formed from the blood, it must contain more oxidized inorganic salts and less deoxidized salts than the blood. But if oxidation still continues, the inorganic constituents, which become perfectly oxidized by the inspired oxygen, must finally be removed from the body, as they are of no further use in it. Thus we find, in fact, that the inorganic constituents of the fluid and solid excrementitious matters are in a perfectly oxidized state, and are perfectly teleoxidic substances. Experiments upon the inorganic constituents of the blood, flesh, and the excrements, have completely confirmed these suppositions.

The inorganic constituents of the blood of the ox have been examined by MM. Weber and Merk (Appendix VIII.).

In the blood which was used for this purpose, the clot certainly had coagulated from the liquor sanguinis; but they were both carbonized without separation.

Water extracted from the carbonized mass a very large amount of chloride of sodium, carbonate of potash and of soda, and mere traces of phosphate and sulphate of potash.

Muriatic acid removed from it an inconsiderable amount of the phosphates of soda, potash, lime, magnesia, and perphosphate of iron.

The carbonized mass after exhaustion with these solvents yielded, on incineration, the phosphates of soda, potash, lime, and magnesia, with a large quantity of peroxide of iron and a small quantity of silica; it also contained traces of sulphates.

The following are the relative amounts of the inorganic constituents obtained by the three operations:—

In the aqueous solution	3.920 grms.
In the muriatic solution	0.389 ...
On the incineration of the remainder of } the carbonized mass	2.128 ...

We thus see that the amount of oxidized salts in the blood is very large. They consist, however, for the most part of salts soluble in water; and of these chloride of sodium, which cannot be considered as an oxidized salt, is the principal constituent, forming more than half of the whole amount of these salts. When this is taken into consideration, the quantity of inorganic constituents formed by the incineration of the carbonaceous mass after exhaustion with the solvents is larger than that of the salts contained in the blood. Hence the blood is a meroxidic body.

Flesh (that of the horse) has been examined as to its inorganic constituents by M. Weber (Appendix IX.).

Water extracted from the carbonized flesh small quantities

only of metallic chlorides, and of sulphate of potash, a large quantity of alkaline phosphates, but no alkaline carbonate.

Muriatic acid then dissolved a considerable quantity of phosphates from the mass.

The residual carbonaceous mass yielded an ash which also consisted of phosphates.

The following were the relative amounts of inorganic constituents obtained in the analysis:—

In the aqueous solution	3·090 grms.
In the muriatic solution	1·262 ...
On the incineration of the carbonized mass	2·866 ...

Hence flesh, like blood, is a meroxidic substance. The latter apparently contains a comparatively larger amount of teleoxidic matter than flesh. But the aqueous solution of the carbonized blood contains nearly 60 per cent. of chloride of sodium, whilst that of the carbonized flesh contained only very small quantities of alkaline chlorides. On taking this into consideration, the quantity of anoxidic matter in the blood is larger than in the flesh, and that of the teleoxidic matter smaller in the blood than in flesh.

The inorganic constituents of the *liquid* and *solid* excrementitious substances have been determined by M. Fleitmann (Appendix X. and XI.).

The extraordinarily large amount of salts contained in the urine is well known: they exist in it in a perfectly oxidized state. On evaporating the urine and carbonizing the dry residue, with exclusion of the air, water extracts almost the whole of the salts. On treating the carbonized mass, after exhaustion with water, with muriatic acid, a somewhat considerable amount of phosphates is further dissolved, part of which had been separated by the evaporation of the urine.

The carbonized mass remaining after the action of water and muriatic acid, yielded so small a quantity of ash on combustion, as to give rise to the supposition that the inorganic constituents obtained in this way had also existed in an oxidized state in the urine, and had merely escaped the action of the solvents in exhausting the carbonized mass. One of the principal constituents of these inorganic substances was silica, which must have been separated on the evaporation of the urine and heating the dry residue, and have thus become insoluble in muriatic acid. The urine may therefore be considered as one of the perfectly teleoxidic substances.

The quantities of the inorganic constituents obtained in these operations were as follows:—

In the aqueous solution	54·148 grms.
In the muriatic solution	5·085 ...
On the incineration of the carbonized mass	0·352 ...

From the carbonized *fæces* water extracted some chloride of sodium and potassium, a considerable quantity of carbonate and phosphate of potash, a less amount of sulphate of potash, and a very large quantity of free potash.

But the quantity of inorganic constituents dissolved by muriatic acid was very large. It consisted of much phosphate of lime and magnesia, phosphate of potash and soda, a very small amount of sulphate, silicate of potash, and a very small quantity of peroxide of iron.

The exhausted carbonized mass on incineration yielded no very inconsiderable quantities of fixed constituents, the principal of which, however, was silica in the form of sand (*sandartige Kieselsäure*). It moreover contained a tolerably large amount of lime and magnesia, with a small quantity of phosphate of potash and soda, peroxide of iron and silica.

The relative quantities in the different solutions and in the remaining carbonized mass were as follows:—

In the aqueous solution	1·933 grms.
In the muriatic solution	6·493 ...
On incineration of the carbonized mass	1·996 ...

But on deducting from the latter amount that of the silica and sand, we only obtain somewhat more than 1 grm., and hence the solid excrements may be considered as belonging to the teleoxidic substances.

Whilst those inorganic oxidized constituents which are soluble in water are separated by the urine, those which are insoluble pass off as *fæces*. In proportion as digestion in the body goes on in a normal state, and the less the excess of nutritive substances introduced into it, the amount of imperfectly oxidized constituents of the *fæces* must be less, and the quantity of teleoxidic substances larger. It is therefore probable that, by the chemical investigation of the *fæces*, a conclusion may be formed as to the proper or defective manner in which digestion is carried on. The fact that the inorganic constituents of the liquid and solid excrements, especially the phosphates, exist in them in the oxidized state, is the principal reason of their use as manure, which must be more fit for this purpose in proportion to the amount of teleoxidic substances of which it consists.

As the inorganic substances from animal bodies are added to plants in the form of manure, the process, in which they are first deoxidized and then again oxidized, recommences.

The examination of other parts of the animal body by the method described is of some interest. As regards the *bones*, it is well known that all their inorganic constituents may be extracted by dilute muriatic acid, so that the cartilage remains in a pure state: likewise when they have been heated to redness with exclusion of the air, muriatic acid dissolves the inorganic salts, and leaves pure carbon. We know that bone-black, as it is called, may be decomposed in this manner.

Thus the bones, like the liquid and solid excrementitious matters, contain the larger amount of their inorganic salts in a perfectly oxidized state, and entirely belong to the teleoxidic substances. This explains the fact that ground bones form one of the best manures.

The *bile* (ox-bile) has been examined by M. Weidenbusch in the same way (Appendix XII.). Likewise, in this case, the surprising result has been arrived at, that the inorganic constituents, as in the excrements, exist almost wholly in the oxidized state, and that the bile is therefore a teleoxidic substance.

By far the larger portion of the inorganic constituents of the carbonized mass of the bile is extracted by water. It dissolves principally the carbonate, phosphate, and sulphate of soda, with chloride of sodium; the quantity of salts of potash dissolved is, however, very small.

Muriatic acid extracted from it a small quantity of phosphate and a trace of silicate. The bases were principally lime, soda, potash, magnesia and the protoxides of iron and manganese.

The washed residue yielded a very small quantity of ash on incineration, which contained a remarkably large amount of sulphuric acid, and but little phosphates. The bases were principally soda, potash, magnesia and lime.

The following are the quantities of the inorganic constituents which were obtained in the various operations:—

In the aqueous solution	16·018 grms.
In the muriatic solution	0·869 ...
On the incineration of the carbonized residue	0·7445 ...

It is thus evident that the inorganic constituents of the bile exist in an almost complete state of oxidation, precisely as in the urine.

The next question is whether the *milk* is also an excretion, like the urine, and whether the whole of its inorganic constituents exist in a perfectly oxidized state. It appears, however, from M. Weber's experiments upon the milk of the cow, that in addition to numerous perfectly oxidized salts, it also contains a considerable amount of inorganic constituents in a

deoxidized state, and is therefore a meroxidic substance (Appendix XIII.).

The aqueous extract of the evaporated and carbonized milk contains a large amount of alkaline chlorides, with phosphate and carbonate of potash.

The muriatic extract of the carbonized mass contains a large amount of phosphate of lime, with small quantities of the phosphates of magnesia, potash and soda.

On the incineration of the exhausted carbonized mass, further large quantities of the phosphates of lime, potash, soda and magnesia were obtained.

The following are the relative proportions of the inorganic constituents obtained in the various operations:—

In the aqueous solution	7·125 grms.
In the muriatic solution	6·621 ..
On the incineration of the remainder of } the carbonized mass }	7·109 ..

Thus the milk contains a considerable amount of both oxidized and unoxidized inorganic constituents. It cannot under any circumstances be arranged in the same class as the urine, and like the latter, be considered as an excretion. It is a meroxidic compound.

In *eggs* (hens' eggs) the inorganic constituents are in a more oxidized state in the albumen, whilst they are in a more unoxidized state in the yolk, as appears from the experiments of M. Poleck (Appendix XIV. and XV.). These experiments, however, were made long ago in my laboratory; the methods adopted were consequently imperfect, and they require repetition. This is the more necessary, since the results of these experiments appear to be in opposition to those which have been obtained in almost all the others. For all those meroxidic substances which have been examined, such as peas, rape-seed and wheat, moreover the blood, flesh and the milk, contain a large amount of the so-called proteine compounds. In teleoxidic substances, the excrements and the bile, the proteine compounds are entirely absent; and in the straw of peas, rape and wheat, they exist in small quantity only, corresponding to the amount of meroxidic substances found in them. It is probable therefore that the proteine compounds, when freed as perfectly as possible from all the teleoxidic substances which accompany them, are in many cases perfectly anoxidic substances. The only results opposed to this view, are those obtained by M. Poleck, as regards the albumen of eggs, which contains a small quantity of anoxidic substances.

Method of examining the Inorganic Constituents of Organic Substances.

Vegetable substances are first freed as completely as possible from all foreign admixtures; solid animal matters are exposed to a gentle heat, to remove the greater part of the water they contain; and animal liquids are to be evaporated to dryness at a gentle heat. In this state the substances are placed in a Hessian crucible, the lid of which is furnished with a hole in the middle, the joints between the lid and the crucible are carefully luted with clay, and the crucible then placed for a long time in a hot place, and, if possible, all the remaining moisture expelled. It is then exposed to a moderately strong heat in a wind-furnace in a charcoal fire; the gases which escape through the hole are inflamed, and when the flame has burnt out and no more gases are evolved, the hole is closed with an accurate stopper of chalk, the crucible heated at a very low red heat, and then allowed to cool with perfect exclusion of the air.

The examination of the carbonized mass resolves itself into three parts.

PART I.—The carbonized mass is first powdered as finely as possible, then boiled for a long time with water in a platinum dish, filtered and washed with hot water until a few drops of the filtering liquid leave a slight residue only, when evaporated upon platinum-foil. It is scarcely possible to continue washing the mass until the liquid which runs through leaves no residue, because a little phosphate of lime is always dissolved. It is therefore best, when the residue obtained on evaporating the water used in washing is very slight, to test a few drops of the water which runs through with solution of nitrate of silver; if a slight opacity only is produced, which disappears on the addition of nitric acid, this arises from phosphate of lime in solution, and we may be certain that all those constituents which are soluble in water have been removed. If the turbidity does not disappear on the addition of nitric acid, this arises from the water used in washing still containing some chlorides, and the washing must be continued longer.

On boiling the carbonized organic substances with water, as also on evaporating the aqueous extract, I have never been able to detect the evolution of ammonia. Hence, during the carbonization, neither alkaline cyanides nor alkaline cyanates are formed.

In all those substances which have hitherto been examined, the aqueous solution contained chlorides. If the amount present is small, the carbonized mass is easily washed; but if they

form a principal constituent, the carbonized mass frequently requires to be washed for several weeks.

It sometimes happens that the aqueous solution appears of a brownish colour, which depends upon the imperfect carbonization of the substance. Usually the liquid becomes decolorized on concentration, and deposits woolly flakes of carbon, which may be easily separated by filtration, before other substances are deposited from it.

If alkaline carbonates exist in the aqueous solution in any considerable quantity, free alkali is formed by the action of the carbon upon the alkaline carbonate, with the evolution of carbonic oxide. If the aqueous solution be evaporated, the dry mass contains more or less hydrated alkali with alkaline carbonate. By means of a solution of nitrate of silver, we may readily convince ourselves of the decomposition of the carbonic acid in the alkaline carbonates by carbon, since, unless the mixture has been exposed to a very great heat, this reagent does not produce a pure white precipitate in the filtered aqueous solution, but a more or less brownish precipitate is formed, which contains oxide of silver as well as the white carbonate of silver.

Decomposition of the alkaline carbonates ensues principally when the organic substance during its carbonization evolves a very large quantity of olefiant gas. Hence it takes place to a great extent when such seeds as contain a large amount of a fatty oil are carbonized, as rape-seed; it occurs to a much less extent on the carbonization of other seeds which do not contain any remarkable quantity of fatty oil, as peas, and likewise with the herbaceous parts of plants.

Unfortunately my attention was not directed to this circumstance until all the experiments described in the Appendix were entirely or partly finished. The alkali among the constituents of the aqueous extract is therefore assumed as existing in the state of hydrate.

It is therefore advisable to pass a current of carbonic acid gas through the aqueous solution before evaporating it, so as to convert the hydrated alkali into the state of alkaline carbonate.

The aqueous solution is then evaporated to dryness in a platinum capsule. It usually happens that when the liquid is very concentrated, it becomes more or less turbid from the separation of phosphate of lime which was in solution. When the liquid has been evaporated to a small volume, the phosphate of lime which has separated may be removed by filtration; it is however difficult to free the aqueous solution from it perfectly in this way; in fact in some cases it cannot

be done, especially when it contains a large amount of silica, which also separates on the concentration of the liquid. It is therefore best to evaporate the whole of the liquid to dryness, and to separate the phosphate of lime in the course of the analysis.

The residue is moderately heated until its weight remains constant. If it be heated too strongly, decomposition of the carbonates contained in it may readily occur, as when silica and phosphates are present carbonic acid is expelled, which in fact partly occurs, according to Heintz*, during evaporation. After determining the weight of the dry residue of the aqueous solution, it is analysed as follows:—

The quantity of carbonic acid is first estimated by means of pure nitric acid in a suitable apparatus. This is obtained by the loss in weight of the apparatus. If, during this operation, the silica has become separated in the acid liquid, it is removed by filtration, and the chlorine precipitated from the filtered liquid by solution of nitrate of silver. The excess of silver is removed from the liquid after the separation of the chloride of silver by muriatic acid, and it is then evaporated to dryness in a porcelain capsule. The dry residue is moistened with muriatic acid and treated with water. Some silica usually remains undissolved, which, when added to that previously obtained, gives the whole amount contained in the aqueous solution.

The liquid separated from the silica is treated with ammonia, by which the entire amount which has been taken up by the water is precipitated. It is filtered, washed, dried, heated to redness, and weighed. Its weight, when deducted from that of the residue after evaporation to dryness, yields the true weight of the aqueous extract. It is mixed with the carbonized mass which has been exhausted by water.

The liquid is then treated with oxalic acid. In very few cases only is a precipitate of oxalate of lime thrown down, in most cases it is not so. The oxalate of lime is determined as carbonate by the ordinary method.

The filtered liquid is then treated with a solution of chloride of barium. The precipitate consists of phosphate, sulphate and oxalate of baryta. It is filtered and washed. Although the oxalate of baryta is not very difficultly soluble, yet after a little practice we can readily judge when the washing must be discontinued. It is therefore advisable, on precipitating the lime, not to add too large a quantity of oxalic acid, so as to avoid obtaining a too copious precipitate of oxalate of baryta.

The precipitate is treated with dilute muriatic acid; sulphate

* Poggendorff's *Annalen*, vol. lxxii. p. 120.

of baryta is then left undissolved, and its weight must be determined after it has been washed. We thus estimate the amount of sulphuric acid. The muriatic solution of the barytic salt is treated with sulphuric acid, and the precipitated sulphate of baryta separated by filtration. It is then supersaturated with ammonia and muriate of ammonia, and sulphate of magnesia added to precipitate the phosphoric acid. Its weight is calculated from the phosphate of magnesia, after having been heated to redness.

The liquid from which the insoluble barytic salts have been separated by chloride of barium, still remains to be examined. The excess of chloride of barium is decomposed by carbonate of ammonia. If the liquid is very dilute, it must be previously concentrated by evaporation. The carbonate of baryta is separated by filtration, the filtered liquid evaporated to dryness, and the dry residue carefully heated to redness to expel the ammoniacal salts. The remaining mass consists entirely of alkaline chlorides. These are separated in the ordinary way by chloride of platinum. The amount of potash is determined from the weight of the ammonio-chloride of platinum. The amount of chloride of sodium may be calculated from the loss; but it is better to determine it directly as sulphate of soda.

PART II.—This part of the investigation is commenced by boiling the carbonized mass, which has been exhausted by water, with muriatic acid for a long time; it is then filtered and washed with hot water, to which a little muriatic acid has been added, until a few drops of the water used in washing leave no residue when evaporated upon platinum foil. The filtered liquid is evaporated nearly to dryness in a platinum capsule. The weight of the dry residue cannot be determined, on account of the readiness with which the chlorides of iron and magnesium are decomposed by heat. The alkalies usually existed in the dry residue as chlorides, whilst they were contained in the exhausted carbonized mass in the form of phosphates; for when the carbonized mass has been washed with water, and a portion of the latter is treated with nitric acid, no precipitate is produced in this solution by solution of nitrate of silver. In arranging the constituents of the muriatic solution, their sum must be taken, and the alkalies represented in the state of oxides.

I was at first much astonished at finding alkalies present in the muriatic solution, as I had no reason to believe they existed in it. Subsequent investigations, however, which I shall describe in a future paper, have shown that when pyrophosphate of lime and pyrophosphate of magnesia are heated with not too large a proportion of alkaline carbonates, remarkable double

salts, consisting of the earthy phosphates in combination with the alkalis, are formed. Most of them are analogous in composition to the ammonio-phosphate of magnesia, but contain a fixed alkali instead of ammonia. The extremely imperfect decomposition which ensues on fusing the phosphates of lime and magnesia with alkaline carbonates, depends in most cases upon the formation of these double salts.

Their formation is undoubtedly the cause of the quantities of the alkalis being estimated incorrectly, and in too small proportion, in many investigations on the determination of the inorganic constituents of organic substances, because their presence in the muriatic solution could not have been expected.

The mass obtained by evaporation is moistened with muriatic acid and then treated with water. Usually a small quantity of silica remains undissolved, which is separated by filtration and its weight determined. The liquid is then neutralized with ammonia. The precipitate contains phosphoric acid in combination with lime, magnesia and peroxide of iron. It is well known that when phosphate of magnesia has been heated to redness, and is then dissolved in an acid, it cannot be completely precipitated by ammonia: the error, however, arising from this source is of only slight importance; because the carbonized mass has been heated for a very long time with muriatic acid, by which means, as is well known, the pyrophosphate of magnesia is almost entirely converted into the *c*-phosphate of magnesia. The extremely small quantity of magnesia which remains in solution is, however, determined in the further progress of the analysis.

The phosphates which have been precipitated by the ammonia are dissolved in nitric acid, and treated with mercury to separate the phosphoric acid from the bases. The dried mass is treated with water, and the mercurial salt removed from the solution by muriate of ammonia and ammonia; the lime is then precipitated by an alkaline oxalate, and the magnesia by phosphate of soda. When peroxide of iron is mixed with the earths, it is separated by the usual methods. The insoluble mercurial residue containing phosphoric acid is fused with carbonate of soda, and the fused mass treated with water. Should any peroxide of iron then remain undissolved, it is separated by filtration, dissolved in muriatic acid, and precipitated by ammonia. The liquid which has been separated from the peroxide of iron by filtration, and which contains phosphate and carbonate of soda, is supersaturated with muriatic acid and then with ammonia, and the phosphoric acid precipitated by sulphate of magnesia. The amount of phosphoric acid is calculated from the weight of the am-

monio-phosphate of magnesia after it has been heated to redness.

By the analysis of the phosphates precipitated by ammonia, the composition of the precipitated phosphate of lime and the presence or absence of the perphosphate or pure peroxide of iron in the precipitate may be determined. The liquid filtered from the earthy phosphates contains either alkalies and phosphoric acid only, as is the case in the examination of all seeds and animal matters, or it contains lime and magnesia only, and these frequently in very considerable quantities. They existed in the carbonized mass in the form of carbonates; but it is only in those cases in which they are present in considerable quantities, as in the analyses of the straw of peas and rape, that a very distinct evolution of carbonic acid can be perceived on treating the carbonized mass, after exhaustion with water, with muriatic acid. As the carbonic acid cannot be determined directly, it is calculated from the quantity of lime and magnesia found in the liquid filtered from the earthy phosphates.

In the analysis of the carbonized mass of vegetable seeds and animal substances, where, as has been previously mentioned, the liquid filtered from the earthy phosphates contains both alkalies and phosphoric acid, this is treated with chloride of barium; phosphate and, when sulphuric acid is present, sulphate of baryta are then precipitated. Sulphuric acid has, however, very rarely been found in the muriatic extract of the carbonized mass. The phosphate of baryta is separated by filtration, dissolved in muriatic acid, the baryta precipitated from the solution by sulphuric acid, which is then supersaturated with ammonia, and the phosphoric acid precipitated as ammonio-phosphate of magnesia.

The liquid which was separated from the phosphate of baryta contains the alkalies. The excess of baryta is removed by carbonate of ammonia, the filtered solution evaporated to dryness, and the dry mass heated to redness to expel the ammoniacal salts. The alkaline chlorides left are separated by chloride of platinum.

The examination of the second portion can be considerably simplified when sulphuric acid is absent. The muriatic extract is carefully evaporated. If it contains silica, the dry mass is moistened with nitric acid, and the silica separated; mercury and nitric acid are added to the filtered liquid; it is then evaporated to dryness with excess of mercury in the ordinary way, and the bases separated from the phosphoric acid by water.

This simplified method has, however, two disadvantages.

The large amount of chlorine present produces a large quantity of protochloride of mercury, which remains mixed with the protophosphate of mercury; hence this insoluble mixture requires a large quantity of carbonate of soda for its decomposition. The second disadvantage is, that when the earths form the principal constituents of the acid extract, it cannot be accurately determined whether they are combined with phosphoric or carbonic acid.

PART III.—This part of the investigation comprises the determination of the inorganic constituents existing in the carbonized mass after exhaustion with water and muriatic acid, or rather which are produced by oxidation.

This incineration of the carbonaceous mass is accompanied by no small difficulties. I formerly effected it by heating it to redness in a crucible, the lid of which was perforated, and a silver tube, which conducted a current of oxygen to the heated mass, was fitted to the aperture. The incineration succeeded perfectly in this manner; but in a very large number of cases it was impossible to incinerate the carbonized mass merely by the access of atmospheric air without the aid of oxygen. But on combustion in oxygen gas, an elevated temperature is produced and the substance of the crucible is powerfully attacked by the phosphates formed by the oxidation. If a platinum crucible be used, it is completely spoiled when alkaline phosphates and carbon act upon it at an elevated temperature. Nor can a porcelain crucible be used; because when the greater portion of the carbon has been burnt, the alkaline phosphates, being in a state of fusion, dissolve the glaze. This is particularly the case with porcelain crucibles of Meissen manufacture; but even those manufactured at Berlin cannot resist the action, although they are far less acted upon than the former. Silver crucibles do not stand the high temperature, and begin to melt. When the combustion of the carbonized mass is effected in a glass tube heated to redness in a furnace by a charcoal fire, it succeeds tolerably well; but the high temperature produced by the combustion of the carbon in oxygen fuses the alkaline phosphates, and they unite with the glass, so that they cannot afterwards be separated from it mechanically. The combustion was finally effected in small earthenware crucibles, which answered in every respect tolerably well, and were but slightly acted upon by the alkaline phosphates; but one important difficulty still remained unconquered,—a considerable quantity of alkaline phosphate is constantly volatilized. This is very large; and the volatilization cannot in any way be avoided when the combustion is effected in oxygen gas, even when the current is slow.

When the gas was passed through too slowly, combustion did not take place; but as soon as it was allowed to flow more quickly, the oxidation of the carbon occurred with the production of a vivid light, and white fumes were distinctly seen to ascend. For the purpose of examining the volatilized products, the combustion of the carbonized mass was effected in a tubulated glass retort, through the tubulure of which the oxygen was passed, whilst its mouth was immersed in water. The water was found to exhibit powerful reactions of phosphoric acid and alkali.

If a mixture of alkaline phosphate with a large proportion of carbon is artificially prepared, and the mixture burnt in a current of oxygen, the same phenomena as those just described are found to occur, and dense white fumes are seen to ascend.

This volatilization only occurs with alkaline phosphates. On the combustion of a carbonized substance, which after incineration yields earthy phosphates only, volatilization cannot be perceived. Moreover, when earthy phosphates are artificially mixed with carbon, and the mixture burnt in oxygen gas, volatilization does not occur, nor can any white fume be perceived. Hence the earlier experiments which were performed in my laboratory, in which the washed carbonized mass was burnt in oxygen gas, have yielded incorrect results, and must therefore be repeated, as a method has at last fortunately been discovered by which the combustion can be effected without loss. This method, which was proposed by M. Fleitmann, is as follows:—

The carbonaceous mass, after having been exhausted with water and muriatic acid, is dried, and then moistened with a concentrated solution of chloride of platinum. The moist mass is heated in a porcelain crucible or a porcelain capsule, at first gently, so that no moisture may be expelled and no loss occasioned by the spirting of the mass; then more strongly, so that it attains an incipient red heat. This is best effected in a large concave lid of a platinum crucible. If the whole be heated in a porcelain crucible, the incineration requires a far longer time, inasmuch as it takes place more slowly in proportion to the smallness of the surface and the less free contact of the air. A very slow combustion of the carbon ensues, chlorine being simultaneously developed; each carbonaceous particle becomes incandescent, just as in the scintillation of an inflamed vegetable substance. The combustion is extraordinarily increased by constantly stirring the mass with a platinum wire or a small platinum spatula. As fresh portions of the unburnt carbon come into contact with the air, the scintillation recommences. The spirit-lamp may be removed,

yet the carbon continues to glow for a long time; frequently, when the quantities are not too small, for several hours; even portions taken out with the platinum spatula continue to burn. The more concentrated the solution of platinum is, the more readily does the combustion ensue. When the mass ceases to glow on the continued application of heat, and still appears black, it must be again moistened with chloride of platinum. The less carbon the residue contains, the more readily does the combustion ensue. If the solution of platinum is concentrated, twice moistening and heating are usually sufficient to produce the combustion of the whole of the carbon. A second moistening with the solution of platinum is not strictly requisite. After the mass has been once moistened and then heated to redness, the residue may be treated with *aqua regia* and evaporated, so as to render the platinum again active; however, this is a somewhat tedious proceeding; great care must also be taken that no loss is occasioned by the spiriting of the mass. As the whole of the platinum used is usually recovered, the method does not entail any pecuniary loss; it is therefore preferable to moisten the mass a second time with chloride of platinum.

When all the carbon has been burnt, the residuary mass is of a pure ash-gray colour. Before it is treated with muriatic acid, it must be heated to redness in a porcelain crucible in a current of hydrogen gas, so that the double compounds of the alkaline chlorides with the chloride of platinum may be completely decomposed, which can only be accomplished with difficulty with large quantities by merely heating them in atmospheric air. If the treatment with hydrogen gas be omitted, the subsequent separation of the platinum from the muriatic solution of the ash is accompanied with several inconveniences. The mass, after having been treated with hydrogen gas, is digested for a long time with muriatic acid in a flask, the residue separated by filtration, and washed with water containing muriatic acid. The resulting solution, which usually contains the same constituents as the muriatic extract of the carbonized mass, is analysed in exactly the same manner.

The platinum remaining undissolved, which is also mixed with the sand and silica of the organic substance, may be separated from these substances by two methods. The whole is either boiled in a platinum capsule with a solution of carbonate of soda, which dissolves the silica, leaving the platinum and the sand, which may then be separated by *aqua regia*; or the mass is treated at once with *aqua regia*, when the sand and silica remain undissolved, and may be separated from each other by boiling with a solution of carbonate of soda. The

$$\frac{d^2u}{dx dy} = \frac{d^2u}{dz^2} + a \frac{du}{dz}, \quad \dots \quad (3.)$$

$$\frac{d^2u}{dx dy} = a \frac{du}{dz}. \quad \dots \quad (4.)$$

the value of a being either zero or unity.

I have not been able to discover that these are capable of further simplification; for which reason in the title of this communication I have called them fundamental forms. If you think this subject worthy of a place in your Journal, I will, in a future communication, add to it a few remarks on the integration of the equations here considered.

1. To transform

$$a \frac{d^2u}{dx^2} + b \frac{d^2u}{dy^2} + C \frac{d^2u}{dx dy} + A' \frac{du}{dx} + B' \frac{du}{dy} + Du = 0,$$

the general partial differential equation of the second order with constant coefficients.

Changing the variables by means of the assumptions

$$\xi = x + gy, \quad \eta = x + g'y,$$

we have

$$\begin{aligned} 0 = & (a + bg^2 + Cg) \frac{d^2u}{d\xi^2} + (a + bg'^2 + Cg') \frac{d^2u}{d\eta^2} \\ & + (2a + 2bgg' + C \cdot g + g') \frac{d^2u}{d\xi d\eta} + (A' + B'g) \frac{du}{d\xi} \\ & + (A' + B'g') \frac{du}{d\eta} + Du. \end{aligned}$$

Now as g and g' are arbitrary, subject only to the condition of being unequal, we may assume them to be the roots of $a + Cg + bg^2 = 0$, unless $C^2 = 4ab$. By this assumption the first and second terms of the last equation will disappear, and therefore the equation will be reduced to the form

$$0 = C \frac{d^2u}{dx dy} + A' \frac{du}{dx} + B' \frac{du}{dy} + Du. \quad \dots \quad (5.)$$

Let us transform this equation by assuming $u = ve^{lx+my}$; by this means it becomes

$$\begin{aligned} 0 = & C \frac{d^2v}{dx dy} + (Cm + A') \frac{dv}{dx} + (Cl + B') \frac{dv}{dy} \\ & + (Clm + A'l + B'm + D)v; \end{aligned}$$

and as l and m are arbitrary it is always allowable to assume $Cm + A' = 0$, $Cl + B' = 0$, by which it is reduced to the form

$$0 = C \frac{d^2u}{dx dy} + Du;$$

and this by writing $a'x$, $-b'y$ for x and y ; and assuming $Ca = a'b'D$, takes the form (1.); to which, therefore, the general equation proposed can always be reduced when C^2 is not $= 4ab$.

2. But when $C^2 = 4ab$, then as we cannot make the above assumptions for g and g' , we may suppose $g = 0$, and $a + Cg' + bg'^2 = 0$; it will then be found that the third term of the first transformed equation in art. (1.) will vanish of itself, and thus the transformed equation is of the form

$$0 = a \frac{d^2u}{dx^2} + A' \frac{du}{dx} + B' \frac{du}{dy} + Du. \quad . \quad . \quad (6.)$$

If we proceed with this as with (5.) it becomes

$$0 = a \frac{d^2v}{dx^2} + (2al + A') \frac{dv}{dx} + B' \frac{dv}{dy} + (al^2 + A'l + B'm + D)v;$$

and as l , m are arbitrary, we can always make the second and last terms of this to disappear; by which means it takes the form

$$0 = a \frac{d^2u}{dx^2} + B' \frac{du}{dy},$$

which may be further reduced, as before, to the form (2.).

3. To transform

$$\begin{aligned} a \frac{d^2u}{dx^2} + b \frac{d^2u}{dy^2} + c \frac{d^2u}{dz^2} + A \frac{d^2u}{dydz} + B \frac{d^2u}{dx dz} + C \frac{d^2u}{dx dy} + A' \frac{du}{dx} \\ + B' \frac{du}{dy} + C' \frac{du}{dz} + Du = 0, \end{aligned}$$

the general partial differential equation of the second order with three independent variables.

Change the variables by the assumptions

$$\xi = x + gy, \quad \eta = x + g'y, \quad \zeta = x + hy + kz.$$

This gives

$$\begin{aligned} 0 = (a + bg^2 + Cg) \frac{d^2u}{d\xi^2} + (a + bg'^2 + Cg') \frac{d^2u}{d\eta^2} + (a + bh^2 + ck^2) \\ + Ahk + Bk + Ch) \frac{d^2u}{d\xi^2} + (2a + Cg' + 2bg' + C.h + Ag' + B.k) \\ \frac{d^2u}{d\eta d\xi} + (2a + Cg + 2bg + C.h + Ag + B.k) \frac{d^2u}{d\xi d\zeta} \\ + (2a + C.g + g' + 2bgg') \frac{d^2u}{d\xi d\eta} + (A' + B'g) \frac{du}{d\xi} \\ + (A' + B'g') \frac{du}{d\eta} + (A' + B'h + C'k) \frac{du}{d\zeta} + Du. \end{aligned}$$

Now as g and g' are arbitrary, subject only to the condition of being unequal, and hk are also arbitrary, subject only to the condition that k must not be zero; if C^2 be not equal to $4ab$, we may assume gg' to be the roots of $a + Cg + bg^2 = 0$, and hk such as to make the 4th and 5th terms of the above equation vanish. By this method the reduced equation takes the form

$$0 = c \frac{d^2 u}{dz^2} + C \frac{d^2 u}{dx dy} + A' \frac{du}{dx} + B' \frac{du}{dy} + C' \frac{du}{dz} + Du. \quad (7.)$$

This equation we may now further transform by writing $ve^{ix+my+nz}$ for u ; by which means we obtain

$$0 = c \frac{d^2 v}{dz^2} + C \frac{d^2 v}{dx dy} + (mC + A') \frac{dv}{dx} + (nC + B') \frac{dv}{dy} + (2nc + C') \frac{dv}{dz} + (cn^2 + Cmn + A'l + B'm + C'n + D)v.$$

Now as l, m, n are absolutely arbitrary, we can always so assume them as to make the 3rd, 4th, and 6th terms of this equation vanish. The reduced equation then takes the form

$$0 = c \frac{d^2 u}{dz^2} + C \frac{d^2 u}{dx dy} + C' \frac{du}{dz},$$

which may be further reduced, as before, to the form (3.).

4. But if $C^2 = 4ab$, we cannot then assume g and g' to be the roots of the equation $a + Cg + bg^2 = 0$; let us therefore in this case assume $g = 0$, and g' such that $a + Cg' + bg'^2 = 0$; and hk as before: it will be found that by this means the 2nd, 4th, 5th and 6th terms of the first transformed equation in art. (3.) vanish, which therefore takes the form

$$0 = a \frac{d^2 u}{dx^2} + c \frac{d^2 u}{dz^2} + A' \frac{du}{dx} + B' \frac{du}{dy} + C' \frac{du}{dz} + Du. \quad (8.)$$

This equation we may now further reduce by the same method as was employed in reducing (7.), and it thus becomes

$$0 = a \frac{d^2 v}{dx^2} + c \frac{d^2 v}{dz^2} + (2al + A') \frac{dv}{dx} + B' \frac{dv}{dy} + (2cn + C') \frac{dv}{dz} + (al^2 + cn^2 + A'l + B'm + C'n + D)v.$$

Now as l, m, n are absolutely arbitrary, we may assign such values to them as to make the 3rd, 5th, and 6th terms of this equation disappear; which being done, it takes the form

$$0 = a \frac{d^2 u}{dx^2} + c \frac{d^2 u}{dz^2} + B' \frac{du}{dy}.$$

But if we change the variables of this equation by writing $\xi = x + fz$, $\zeta = x + f'z$, it becomes

$$0 = (a + cf^2) \frac{d^2 u}{d\xi^2} + (2a + 2cfj') \frac{d^2 u}{d\xi d\zeta} + (a + cf'^2) \frac{d^2 u}{d\zeta^2} + B' \frac{du}{dy};$$

in which we are at liberty to suppose f, f' to be the roots of $a + cf^2 = 0$, which reduces it to the form

$$0 = B \frac{d^2u}{dx dz} + B' \frac{du}{dy},$$

which may be further reduced, as before, to the form (4.).

Thus it has been proved that (1.), (2.), (3.), (4.) are the fundamental forms of partial differential equations of the second order with constant coefficients, for two and three variables. If there be more variables, it is evident the method here employed will still apply, and enable us to reduce them to fundamental forms. I have not had occasion however to pursue the method beyond what is here done; nor have I applied it to equations of the third and higher orders, though I am inclined to think that with some modification it would enable us to reduce them to fundamental forms.

Sheffield, May 18, 1849.

III. *Cliff Section of Lundy Island, from the Sugar-Loaf to the Devil's Limekiln.* By the Rev. D. WILLIAMS*.

THE specimens to which the following descriptions refer, were collected and numbered on the spot, and occupy, as near as may be, the relative positions assigned them on the section.

No. 1. Granite, often of the porphyritic variety, a ternary compound of mica, quartz, and felspar only.

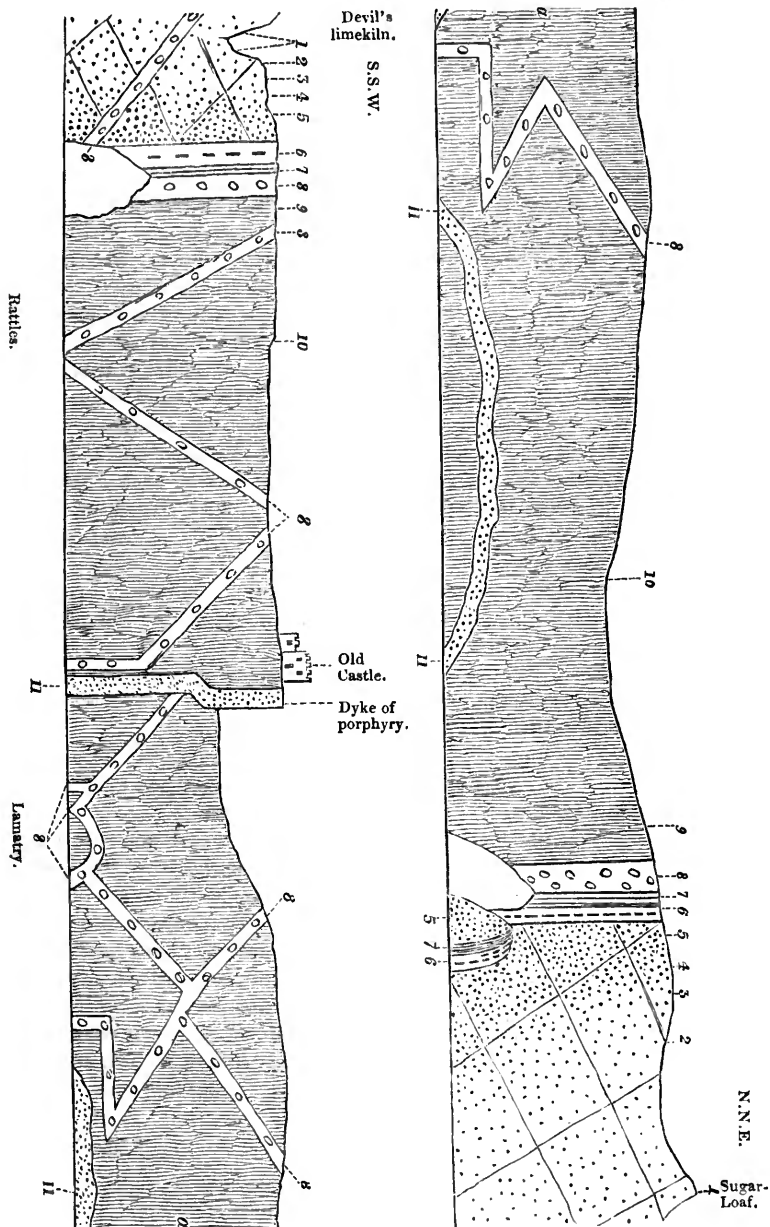
2. Gray granite, differing only from the former by the accession of rare and remote specks and crystals of foliated hornblende, which apparently replace the mica to the amount only of such accession.

3. Gray granite, more syenitic, of a greenish tint, not so distinctly crystalline granular as the last, at times calcareous, and the mica in a greater degree absent.

4. Gray granite, with a less shade of green, no hornblende, slightly calcareous, mica none or scarcely perceptible, the quartz crystalline granular, and the felspar in well-defined and confusedly crystalline arrangement.

5. Gray, compact, or finely granular granite rock, with no trace of hornblende, the mica absent or extremely minute and scarcely distinguishable, but otherwise compounded of felspar, quartz, carbonate of lime, and calcareous spar; the two latter in such augmented proportions, as to cause the rock to effervesce briskly or feebly at nearly all points on being tested by acids.

* Communicated by the Author; being the substance of a memoir communicated to the British Association, &c. held at Swansea in 1848.



6. Dark olive-gray hornblende porphyry, passing into a black bottle green colour with disseminated crystals of calcareous spar, at times remote, at others so thickly grouped together as almost to usurp the base, and nearly to touch each other.

7. Light and olive-gray compounds of clay, felspar, quartz and lime, parted out in inch-thick layers and lesser laminations of schist by delicately thin plates of fibrous calcareous spar, passing into a calcareous trappean foliated marl, and marl rock, sometimes more thickly laminated and without the calcareous spar partitions, at others a clay schist. At the foot of the section on the N.N.E., where 6 and 7 is as if wedged in, and curves out in No. 5*, a conglomerate of fine gravel in a base of felspathic schist or greenish-gray volcanic ash, pertaining rather to No. 5 than to 6 or 7, is distinctly seen.

8. Black, finely granular crystalline hornblende trap, feebly calcareous, rendered somewhat porphyritic by crystals of glassy felspar, so nearly black as only to be detected by the light reflected from their cleavaged facets; it also contains sparingly, a green mineral like olivine, more frequently iron pyrites, at times white on exposure; and commonly small white spherules of calcareous spar.

9. Unctuous clay schist, evidencing no alteration from its contact with No. 8, differing little or nothing in composition and texture from much of No. 7.

10. Flinty, talcose clay, and felspathic schists and slates, in indefinite alternations.

11. Buff and ochreous rusty-coloured porphyry.

N.B. The unshaded parts represent a lofty cavity at either extremity, caused by the more ready decomposition of the calcareous products.

It is a notable fact, that after the confines of the slate and granite at Lundy, a distance of half a mile, we have not a vestige of a granite vein; the same intermediate substances, Nos. 6, 7 and 8, are met with at either extremity, and at intervals between them, and the immediately bounding schist and slate show no amount of alteration, or so little, that regarding those intervening substances, and their mineral relations with, and transition into, the granite, the inference that the schists had metamorphosed the granite or the original granite lava, is far more probable and defensible than an inverse supposition.

From Nos. 1 to 10 inclusive, we had so many graduated

* This remarkable feature in the section is at nearly the base of the cliff, and is sometimes quite obscured by shingle, which shifts there very much at times.

stages of mineral variation, between the extreme terms of typical granite passing by calcareous granite, syenite, black compact hornblende trap, greenstone porphyry, and greenstone ash or mud, into clay schist and crystalline slate (the proximate effects of a great physical constant), linked together in such a chain of mutual relations and dependences as demonstrated their common source and origin.

The author had met with the same connected mineral series in every ancient volcanic group of Devon and Cornwall, from the focal granite lava to the finely lævigated and impalpable mud, oftentimes in more full and circumstantial particulars than were detailed at Lundy Island; but, on the other hand, the daily washed and unambiguously disclosed sea-cliff sections of the latter, satisfactorily explained to his mind the intervalled but reiterated occurrence of those enormous masses of hypersthene and greenstone on the flanks and immediate confines of the several granite domes, and which often appeared to pass by syenite into the granite, but whose relations to it otherwise were concealed. Wherever he had met with those masses of greenstone, &c., he had never met with granite veins penetrating the bounding rocks, whereas they were uniformly more or less present in the intervals of their occurrence. There, as at Lundy, the bounding sedimentary rocks in the one case showed little or no amount of alteration, in the other they were changed to a vast amount and extent, and exhibited all the varying phases or stages of reduction, from perfect fusion to semifusion and incipient induration.

The remarkable proportions of lime in certain of the Lundy series, constituting, as it did in some of the terms, far beyond a moiety of the component ingredients, gave undeniable proof of the occasional presence of lime in granite and its cognate thermogenous rocks; so that lime is not invariably so deficient a substance in this class of mineral compounds as had commonly been supposed; while the instances he adduced naturally suggested the inquiry, why such deficiency should exist in the majority of cases.

He regarded it as an altogether fabulous assumption, and a necessary corollary of the day-dream of the solar, or fire-mist origin of granite, that at any period of the earth's history, lime should have been in a less proportion than it bears at present to the other elementary earths. Such fabulous origin of granite, combined with the supposed absence of lime in it, sufficiently explains the contrary supposition which has been advanced; while the sequel hypothesis, which attributes the present amount of lime in nature to the polyparia and mol lusca,—in other words, that creatures whose existence at all

depends upon a pre-provision of lime in the medium in which they are destined to live, should originally have created it out of nothing, very admirably sustains the unities of the fiction.

He had learned in Devon and Cornwall, that the several granite bosses there, pertained to and were inseparably connected with certain well-defined divisions, of what he termed the Ocrynian group. Those granites he regarded severally as so many ancient submarine volcanic centres, consecutively generated, and raised to their present positions by "elevation crater movements," each one standing out now in lofty and august relief above its mineral relations and dependences, the tor-crowned type and head of its own volcanic department.

Those inseparable associates and dependences on granite, were syenites, porphyries, hypersthènes, serpentines, greenstones, greenstone ash and tuff, clay schists, crystalline slates, and *coral built and chemically precipitated limestones*.

In every instance the author had met with, all the limestones and calcareous rocks which had been invaded and acted on by the vein-like processes of fusion, or by active heat, had lost either the larger proportion or the whole of their lime; in other words, the lime and carbonic acid had been driven off by heat.

Bearing on this fact, the vast masses of travertine which have been precipitated from water in subaërial volcanic districts, must have been at the expense of some other rocks or products, from which it had been abstracted by it, a result no doubt of the eager affinities which are known to subsist between lime and water.

These simple and familiar principles he considered quite adequate to explain the relative absence or low per-centage of lime in granite, and in its many metamorphosed and disguised relations, taking them as submarine volcanic products. The lime, driven off by the reduction of calcareous rocks, or availing itself of the freedom it acquired in the condition of fluid lava, would readily part from its less attractive associates to unite with an element of more powerful affinities, which would serve as its vehicle of translation beyond the cincture or radius of the deadly volcanic emanations and their "azoic and protozoic" deposits, to remoter and more genial regions, where it would minister to the requirements of multiplying myriads of coral architects and other zoophytous, molluscous, crustacean, and vertebrate creatures, which out of that constant provision and supply of materials would in time construct rock masses of such corresponding scale and dimensions, as to contribute to and subserve the more final purpose of terrestrial restoration or repair.

Another irresistible induction presented itself in the Lundy series. The most fastidious mineralogist, if he admitted granite to be a physical substance, and not a metaphysical abstraction, must concede that the specimen No. 2 on the section, was granite modified by a very trifling accession of hornblende. But this necessary admission will conduct him by a series of insensible gradations to the inevitable conclusion, that the calcareous dark green porphyry and the black hornblende trap or greenstone are nothing else than granites in disguise, or granitic matter masked in colour and modified in substance, by the accession of varying proportions of hornblende and lime, and no doubt other adventitious minerals.

The cabalistic traditions of cosmogony which leaven the whole lump of geology at the present hour, and which rigorously excluded a particle of hornblende and lime from granite, nevertheless included (no doubt from their universal association) slate and schist in the category of granite and the primitive formations. Yet what geologist, he asked, in the present day, would venture to talk of primitive or primary slate and schist?

If these slates and schists, then, were demonstrably neither azoic, protozoic, prinital, primitive or primary—if, in truth, they were of all dates, from the most ancient to the newer pliocene, the granites and their congeners were the same. There was a negative fact, too, which (in the universality of its negation, accompanied, as it was, by such an accumulation of probable and circumstantial testimony as amounted to the best positive evidence,) led to the same conclusion. If granite were a primary and independent substance, a sort of mineralogical fact but a metaphysical abstraction, as the cosmogonists still hold, it assuredly ought to occur, at times at least, unaccompanied by, and apart from, its present universal associates, which it assuredly does not in any portion of the globe which has hitherto been explored. On the contrary, the author was prepared to show, that *the same predominant mineral types, or the same average proportion of constituents, characterized alike the granites, porphyries, serpentines, greenstones, slates and schists, of any well-defined division of the Devon and Cornish group.*

In the Lundy series we eliminate the important truth, how egregiously we may be, and doubtless have been, imposed upon by the varying accession of one deceptive compound; the minimum amount of hornblende in No. 2 demonstrating, by the aid of the intermediate gradations, that its maximum of imposition in Nos. 6 and 8 is simply a question of degree; while from the conditional fact or accident of its absence in

No. 1, we learn what fabulous results have been assumed, and what visionary fabrics have been imagined, and dreamy phantasies indulged in, of prinital fire mist, and secular refrigeration, and azoic and protozoic rocks, and gelatinous monads, and progressive development, and strata identified in universal formations, and the long catalogue and concatenation of chimæras and romance which in the present day is either substituted for sober science, or which so poisons its well-heads that its current throughout is corrupted and debased by it; corollaries essentially consequent on "the folly," as Playfair terms it, "of attempting to explain the first origin of things," and which implicates certain geologists in the something more than folly, viz. that while they ostentatiously parade their trite and broad phylactery, "denique non belle et probabiliter opinari, sed certo et ostensive scire," they assume a definite sense and terms, and draw physical land-marks, out of what are merely convenient cabinet mineralogical distinctions, not definitions in nature, but convenient distinctions in a constantly running and blending series, in which it is impossible to limit a genus or a species.

Bleadon, April 20, 1849.

IV. *Note on the Geological Structure of the Asturias, particularly in reference to the Nummulitic Eocene, and the Carboniferous Palæozoic Rocks of that Province (extracted from a letter of M. E. DE VERNEUIL addressed to Sir RODERICK I. MURCHISON*)*.

IN a tour which he is now making in Spain, M. Ed. de Verneuil has observed, that on the frontiers of the provinces of Asturias and Santander, the nummulitic formation overlies all the true cretaceous rocks, and that no form of the genus *Nummulina*, D'Orb., ever occurs in them; thus fortifying the generalization recently announced by Sir Roderick Murchison, deduced from a study of the Alps, Apennines, and Carpathians; viz. that the nummulitic group of Southern Europe, and which extends over such an enormous area in Asia, is the true Eocene tertiary of geologists. The cretaceous or uppermost secondary rocks of the north of Spain consist of two great stages, the lower of which is the Diceras limestone, and the uppermost a group of limestones and argillaceous sandstones, &c. with Hippurites, Radiolites and *Orbitolites*. The last-mentioned bodies have been supposed to be Nummulites; and hence has arisen the mistake of supposing, that Nummulites and Hippurites are associated in those

* Communicated by Sir R. I. Murchison.

limestones of the south which represent the chalk of the north of Europe. Above the zone of Orbitolites, is a yellowish limestone with *Spatangi*, which representing the upper chalk of the north, is widely developed at Santander, between the town and the lighthouse.

The nummulite limestone then follows as the next deposit in ascending order, and is overlaid, as in the Alps, by sandstone, &c. In this formation M. de Verneuil discovered, in addition to Nummulites, the *Serpula spirulæa*, *Conoclypus conoideus*, *Ostrea crassissima* or *gigantea*, fossils so well known in the nummulite rocks of the Alps, Vicentine and Crimæa. This eocene group, whose fossils are so distinct from those of the cretaceous system, nevertheless follows all the flexures and dislocations of the latter, just in the manner recently described by Sir Roderick Murchison in the Alps and Apennines. The same relations, zoological and stratigraphical, are said (on the authority of Don Amalio Maestre the Inspector of Mines of the Asturias) to extend from Aragon towards Valencia.

In describing the principal features of the carboniferous rocks of the Asturias (some of the peaks of whose limestones rise to upwards of 8000 feet above the sea), M. de Verneuil shows, that the chief seams of coal are fairly intercalated with courses of limestone and schists charged with the well-known British species *Productus antiquatus*, *P. punctatus* and various marine fossils. In this and in other overlying stages with conglomerates, &c. containing coal, there is, the author observes, no sandstone or schist which can have served as a soil on which jungle or marsh plants can have grown; and seeing the alternation of the fossil vegetables with marine deposits, he concludes that these coal-fields, like many others, and particularly those of the Donetz in Russia described by Sir R. Murchison and himself, were formed in estuaries of the sea by the transport and subaqueous deposit of terrestrial spoils, and are not referable to the same origin as certain carboniferous strata of the British Isles, America, &c., the coal beds of which are supposed to have been formed of vegetable masses *in situ*. In the second stage of this carboniferous formation, M. de Verneuil discovered, that courses of calcareous schists were loaded with *Fusulinæ*—a point of very great interest; since these foraminifera have been described in the mountain limestone of Southern Russia*, and were subsequently discovered by M. de Verneuil in the carboniferous limestone of the United States of America. Their occurrence at this intermediate station in Spain is therefore highly interesting in extending our acquaintance with the uniformity of distribution

* See Russia in Europe and the Ural Mountains, vol. i.

of animal life in the palæozoic ages. The coal-fields of the Asturias (of which there are seventy workable seams) seem therefore to be subordinate to the mountain limestone, like those of the north of Northumberland, the south of Scotland, &c. &c.

The Devonian system has been found to abound in the north of Spain, chiefly through the researches of M. Paillette, who has transmitted many of its fossils to France, where they have been described by M. de Verneuil.

The Triassic and Jurassic systems are also stated to be considerably developed in Spain, and like the palæozoic rocks they are highly dislocated.

In conclusion, the author remarks, that the interesting region of the Asturias will soon be better known, first through a very exact geographical map prepared by M. Paillette, particularly in reference to its coal-fields; and next by a general geological map of the province by Don. G. Schultz, on which that gentleman has been occupied during four years, and which is spoken of as a work of great merit.

V. *Analytical Researches concerning Numbers.* By CHARLES JAMES HARGREAVE, Esq., LL.B., F.R.S., Professor of Jurisprudence, University College, London*.

THE theory of numbers is a branch of mathematics which has either been regarded as not falling under the dominion of analysis, or has eluded the researches of those who may have attempted its application; so that, notwithstanding the great powers and singular industry of the eminent men who have devoted themselves to this science, it remains, so far as its processes are concerned, nearly in the condition in which the theory of probabilities would have stood, if the higher analysis had not been applied to it. It abounds with theorems of remarkable elegance; but is destitute of the processes, which alone, in case of need, could render it a practical science.

Legendre, in his treatises on this subject, communicated as the result of an observation of numbers in the first million, a formula for determining the number of primes up to any given limit; and adopting this formula as a law, he was conducted to some theorems involving the application of analysis. (*Théorie des Nombres*, vol. ii. § VIII.) That the formula thus given by him is not really the natural law of primes, is apparent from the circumstance that it involves a constant, or rather

* Communicated by the Author.

two constants, whose values are assigned by experiment, and which can have no natural connexion with the subject; and as these constants are merely calculated so as to give as little error as possible for the small limits within which the observations are confined, there is no *à priori* reason for supposing that their assigned values remain permanent for numbers of an order of magnitude far exceeding the observed limit.

The primary object of the following investigation is the discovery of the laws regulating the occurrence of prime numbers by means of analysis; and it is evident that if this object can be effected by the deduction of a simple analytical law, we shall be in a position to deduce many curious and interesting results connected with the subject.

The power of applying analysis to this subject is based upon a principle, which at present it would be presumptuous to rank amongst recognized forms of mathematical reasoning; to wit, that the real analytical equivalent of the different values of an indeterminate expression is the arithmetical mean of those different values. It is satisfactory, however, to be able to observe that this dogma is not propounded here for the first time; but has appeared, at least as matter of induction, under the high authority of Professor De Morgan. (Camb. Phil. Trans., vol. viii. part 2. No. 15.) The present is not a convenient opportunity for the discussion of this principle; but as it is likely to be introduced in some shape or other into analysis, it is very necessary that the grounds upon which it rests, and the subjects to which it may be lawfully applied, and the manner of its application, should be analysed, and recognized to a due extent. I hope on another occasion to be able to advance some reasons for the conclusion, that this principle may be introduced into mathematics without departing from or unduly extending doctrines heretofore admitted. At present, I shall merely observe that I do not regard it as probable that the principle in question, or any principle analogous to it, is deducible from the fundamental axioms of *algebra*; that it may nevertheless be true in some definable and useful sense; that, so far as experience leads us, no incongruity arises from the application of this doctrine, but that, on the other hand, whenever an analytical equivalent is deduced naturally, it is found to coincide with the arithmetical mean; and lastly, that the subject of this paper affords a favourable case for its application, inasmuch as the results which we propose to obtain are precisely of that *average* character of which results flowing from such a principle might be expected to partake.

In the course of this paper I shall have occasion to employ

a known transcendental integral $\int_0^x \frac{dx}{\log x}$; which, although not used as commonly as logarithmic or trigonometrical functions, has received a distinctive name (logarithm-integral) and symbol ($\text{li}x$), and has been tabulated by Soldner from 0 to .99 and from 1.1 to 1280. The table is given in Mr. De Morgan's *Diff. Cal.*, pp. 662, 663. It will be useful to note the following properties:

$$\int_0^x \frac{x^m dx}{\log x} = \text{li}(x^{m+1}),$$

(except when $m = -1$, in which case

$$\int \frac{x^{-1} dx}{\log x} = \log \log x, \quad \int \frac{x^\epsilon dx}{x} = \text{li}(x^\epsilon), \quad \int \text{li}x dx = x \text{li}x - \text{li}(x^2).$$

The following researches do not imply or assume any knowledge of the observed values or deduced properties of prime numbers, except so far as is expressly mentioned; but I have not on this account thought it necessary to employ symbols in lieu of well-known primes (such as 2, 3, 5, 7 . . .), where the use of the numbers places the expressions in a more familiar form. The symbol p is used to denote a prime number in general; and accents or suffixes are attached when necessary for distinction.

Prop. 1. If

$$P_n = \frac{1}{1^n} + \frac{1}{2^n} + \frac{1}{3^n} + \frac{1}{4^n} + \dots$$

where 1, 2, 3, 4 ... denote the series of *natural* numbers to infinity, then

$$P_n = \left(1 - \frac{1}{2^n}\right)^{-1} \left(1 - \frac{1}{3^n}\right)^{-1} \left(1 - \frac{1}{5^n}\right)^{-1} \dots$$

where 2, 3, 5... denote the series of *prime* numbers to infinity.

If we take the natural numbers beginning with 2, and *strike out* from the series the first prime (2) and all its multiples, then the second prime (3) and all its multiples, and so on through the series of prime numbers, we shall have struck out all the numbers in the following manner: the primes will have been struck out, once; the composites of two primes, twice; the composites of three primes, three times; generally the composites of m primes, m times.

If we now go through the series again, and *restore* 2.3 and all its multiples, 2.5 and all its multiples, 3.5 and all its multiples, and generally $p_1 p_2$ and all its multiples, going through the series of primes, we shall have restored the composites of 2 primes, once; the composites of 3 primes, three times; the composites of 4 primes, six times; and generally the compo-

sites of m primes, $m \frac{m-1}{2}$ times, being the number of combinations of two things in m things.

If we go through the series again, and *strike out* 2.3.5 and all its multiples, and generally $p_1 p_2 p_3$ and all its multiples, we shall have struck out the composites of 3 primes, once; the composites of 4 primes, four times; and generally the composites of m primes, $m \frac{m-1}{2} \frac{m-2}{3}$ times, being the number of combinations of three things in m things.

Repeating this process continually, alternately striking out and restoring, the final result will be as follows:—The primes will have been struck out once and never restored; the composites of 2 primes will have been struck out twice and restored once; the composites of 3 primes will have been struck out three times, restored three times, and struck out once; the composites of 4 primes will have been struck out four times, restored $\frac{4.3}{2}$ times, struck out four times, and restored once; the composites of 5 primes will have been struck out five times, restored $\frac{5.4}{2}$ times, struck out $\frac{5.4.3}{2.3}$ times, restored $\frac{5.4.3.2}{2.3.4}$ times, and struck out once; and generally the composites of m primes will have been struck out in all

$$m - \frac{m(m-1)}{2} + \frac{m(m-1)(m-2)}{2.3} - \dots \pm m \mp 1 \text{ times;}$$

that is simply once; since this expression is $1 - (1-1)^m$ or 1. The general effect therefore of the whole of this process is, that the original series of natural numbers is exactly exhausted.

This process is one of great simplicity, as it involves nothing more than the mechanical process of determining prime numbers by exhausting the composites.

Bearing these considerations in mind, it will be immediately obvious that the proposed series wanting its first term, or $P_n - 1$, is

$$P_n \left(\sum \frac{1}{p_1^n} - \sum \frac{1}{p_1^n p_2^n} + \sum \frac{1}{p_1^n p_2^n p_3^n} - \dots \right);$$

and, deducting both sides of this equation from P_n , and dividing by P_n , we have

$$\begin{aligned} \frac{1}{P_n} &= 1 - \sum \frac{1}{p_1^n} + \sum \frac{1}{p_1^n p_2^n} - \sum \frac{1}{p_1^n p_2^n p_3^n} + \dots \\ &= \left(1 - \frac{1}{2^n}\right) \left(1 - \frac{1}{3^n}\right) \left(1 - \frac{1}{5^n}\right) \left(1 - \frac{1}{7^n}\right) \dots \end{aligned}$$

which is the theorem proposed.

This theorem is arithmetically true, and may be verified approximately by calculation, when n is greater than unity; but for other values of n , whether positive or negative, we do not approximate to arithmetical truth by taking additional terms of the series. If we multiply together a definite number of factors of the form

$$\left(1 - \frac{1}{2}\right)^{-1} \left(1 - \frac{1}{3}\right)^{-1} \left(1 - \frac{1}{5}\right)^{-1} \dots \left(1 - \frac{1}{p}\right)^{-1},$$

we obtain the sum of the reciprocals of consecutive ordinals up to the next prime beyond p exclusive: and besides this, we obtain the reciprocals of an infinite number of scattered ordinals, whose aggregate sum does not diminish without limit as p increases. This may be expressed by saying, that in the equation

$$1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \dots + \frac{1}{\alpha} = \left(1 - \frac{1}{2}\right)^{-1} \left(1 - \frac{1}{3}\right)^{-1} \left(1 - \frac{1}{5}\right)^{-1} \dots \left(1 - \frac{1}{\alpha}\right)^{-1}$$

the two infinities are not the same, and the difference between them is a material consideration.

The proposition above given readily conducts us to expressions for Bernoulli's numbers in terms of prime numbers; which, however, we reserve for future discussion.

Prop. 2. If $\left(\frac{x}{p}\right)$ denote the whole number $\frac{x}{p}$ when x is divisible by p , and the next whole number below $\frac{x}{p}$ when x is not divisible by p , then the continued product $1.2.3.4 \dots x$ or $[x]$ may be expressed by means of its prime factors in the form

$$2^{\left(\frac{x}{2}\right) + \left(\frac{x}{2^2}\right) + \left(\frac{x}{2^3}\right) + \dots} 3^{\left(\frac{x}{3}\right) + \left(\frac{x}{3^2}\right) + \left(\frac{x}{3^3}\right) + \dots} 5^{\left(\frac{x}{5}\right) + \left(\frac{x}{5^2}\right) + \left(\frac{x}{5^3}\right) + \dots} \dots$$

all the primes up to x being employed.

This theorem may be deduced from the considerations put forward in the first proposition; but as I find that it is demonstrated by Legendre (Introduction, p. 10), I omit the proof.

The familiar association of the arithmetical continued product $[x]$ with its algebraical extension $\Gamma(x+1)$, where Γx is a solution of the functional equation $\phi(x+1) = x\phi x$, conducts us naturally to the investigation of an algebraical extension for this continued product when resolved into its prime factors; an investigation which throws us back on the fundamental principles upon which algebraical extensions of this

nature are based. Now in effecting such an extension, we attach a notional continuity to expressions which in their natural sense are not properly continuous, but proceed *per saltus*; and our power to do this arises from the circumstance, that whatever discontinuity in the algebraical sense exists in expressions of the form $1.2.3...x$, $1 + \frac{1}{2} + \frac{1}{3} + .. + \frac{1}{x}$, $\log 1 + \log 2 + .. + \log x$, &c., such discontinuity is itself the subject of a regular and continuous law. The process is an interpolation of all those algebraical forms which are necessary to be interpolated, in order that expressions of this kind may have continuous values for every value of x proceeding by infinitely small increments, instead of proceeding by definite equal increments.

Now prime numbers are not naturally anomalous any more than ordinals are; they are the elements of the ordinals; and the infinite series of natural primes is the framework or skeleton of the series of natural ordinals, the texture superposed upon such framework being manufactured by a law which we can express. The whole series of ordinals

$$1 + 2 + 3 + 4 + 5 + 6 + 7 + 8 + 9 + 10 + 11 + 12 + \dots$$

is formed by the continued multiplication of the several series

$$2^0 + 2^1 + 2^2 + 2^3 + 2^4 + 2^5 + 2^6 + ..$$

$$3^0 + 3^1 + 3^2 + 3^3 + 3^4 + 3^5 + 3^6 + ..$$

$$5^0 + 5^1 + 5^2 + 5^3 + 5^4 + 5^5 + 5^6 + ..$$

$$\vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots \quad \vdots$$

$$p^0 + p^1 + p^2 + p^3 + p^4 + p^5 + p^6 + ..$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

where the sign $+$ is used in the ordinary sense for the purpose of multiplication, but in the result merely means juxtaposition. This is sufficiently apparent from the fact that the above product gives every possible combination of primes and powers and multiples of primes, and cannot give the same number twice, since no one prime or any power of it can be equivalent to any other prime or any power of it. In effect, in the first theorem we resolved the infinite series of ordinals into

$$(1-2)^{-1}(1-3)^{-1}(1-5)^{-1}...(1-p)^{-1}...,$$

which is the algebraical form of the expression last above written.

Let us now denote the natural series of primes by

$$p_1 p_2 p_3 \dots p_y \dots,$$

and suppose ourselves not to have any previous acquaintance with their properties further than necessarily flows from the circumstance that they are the elements of which the natural numbers are composed. We have then by virtue of this fact the equation

$$[x] = p_1 \left(\frac{x}{p_1} \right) + \left(\frac{x}{p_1^2} \right) + \dots p_2 \left(\frac{x}{p_2} \right) + \left(\frac{x}{p_2^2} \right) + \dots \dots p_y \left(\frac{x}{p_y} \right) + \left(\frac{x}{p_y^2} \right) + \dots,$$

where p_y is the last prime in the series of ordinals up to x ; and the question suggests itself, what meaning is to be attached to the second side of this identity, when the first side is used, not in the arithmetical sense, but in the extended sense which is ordinarily understood by the term $\Gamma(x+1)$. What can we

propose as a proper analytical equivalent of $\left(\frac{x}{p_1} \right) \dots$? To this

question it would perhaps be difficult to give a general satisfactory answer; it will be sufficient for the present purpose to

express the analytical equivalent of $\left(\frac{x+\Delta x}{p_1} \right) - \left(\frac{x}{p_1} \right)$; and it

is submitted that if any such equivalent exists, it can only be $\frac{\Delta x}{p_1}$ in the ordinary sense. In this investigation x and p_1 are

symbols to which we do not assign a definite arithmetical value; we know merely that p_1 is a prime. Now $\left(\frac{x}{p_1} \right)$ may differ from the ordinary fraction $\frac{x}{p_1}$ by any one of the quantities

0, $\frac{1}{p_1}$, $\frac{2}{p_1}$, $\frac{3}{p_1}$, ... $\frac{p_1-2}{p_1}$, $\frac{p_1-1}{p_1}$; and $\frac{x+\Delta x}{p_1}$ may in like man-

ner differ from the ordinary fraction $\frac{x+\Delta x}{p_1}$ by any one of the

same quantities; and if we are at liberty to adopt in its simplest form the principle before alluded to,—that the analytical equivalent of an indeterminate expression is the arithmetical mean of all its possible values,—we shall have no difficulty in concluding that the proper expression for the difference in

question is $\frac{\Delta x}{p_1}$; for its value may differ from $\frac{\Delta x}{p_1}$ by any one of a set of quantities the arithmetical mean of which is zero.

It will be observed that in the arithmetical identity of which we are speaking, we are at liberty to consider the series

$$\left(\frac{x}{p_1} \right) + \left(\frac{x}{p_1^2} \right) + \dots$$

as extending *ad infinitum*, without thereby impairing the truth of the equation, or detracting from its purely arithmetical

character; for $\left(\frac{x}{p_1^n}\right)$ vanishes whenever $\frac{x}{p_1^n}$ is less than 1.

Now in applying to the equation the principle of means in the manner above suggested, it is proper to consider it in this extended form; for if we suppose the series to stop so soon as p_1^n exceeds x , then in deducting $\left(\frac{x}{p_1}\right) + \left(\frac{x}{p_1^2}\right) + \dots$ from $\left(\frac{x+\Delta x}{p_1}\right) + \left(\frac{x+\Delta x}{p_1^2}\right) + \dots$, we lose sight of the circumstance that the number of terms in the latter expression is not necessarily the same as that in the former expression; or, in other words, that the mean number of terms in the latter is necessarily greater than the mean number of terms in the former.

Acting upon this principle of means, we equate

$$\left(\frac{x+\Delta x}{p_1}\right) + \left(\frac{x+\Delta x}{p_1^2}\right) + \dots - \left(\left(\frac{x}{p_1}\right) + \left(\frac{x}{p_1^2}\right) + \dots\right)$$

to

$$\Delta x \left(\frac{1}{p_1} + \frac{1}{p_1^2} + \dots \right),$$

which amounts in effect to this; that if we write the first part in the form

$$(x+\Delta x) \left(\left(\frac{1}{p_1}\right) + \left(\frac{1}{p_1^2}\right) + \dots \right),$$

and the other part in the form

$$x \left(\left(\frac{1}{p_1}\right) + \left(\frac{1}{p_1^2}\right) + \dots \right),$$

then for the purpose of subtraction the $\left(\frac{1}{p_1}\right) + \left(\frac{1}{p_1^2}\right) + \dots$ may be regarded as meaning the same thing in both.

Prop. 3. To investigate the law which regulates the occurrence of prime numbers in the ordinal series.

Let x be a prime number occupying the place p_y in the series of primes; and let $x+\Delta x$ be the next prime number occupying the place p_{y+1} in the series of primes. We have then

$$\Gamma(x+1) = \left(p_1^{\left(\frac{1}{p_1}\right)+\dots} p_2^{\left(\frac{1}{p_2}\right)+\dots} \dots p_y^{\left(\frac{1}{p_y}\right)+\dots} \right)^x,$$

$$\Gamma(x+\Delta x+1)$$

$$= \left(p_1^{\left(\frac{1}{p_1}\right)+\dots} p_2^{\left(\frac{1}{p_2}\right)+\dots} \dots p_y^{\left(\frac{1}{p_y}\right)+\dots} p_{y+1}^{\left(\frac{1}{p_{y+1}}\right)+\dots} \right)^{x+\Delta x};$$

the x and $x + \Delta x$ being properly the numerators of the exponents. Now substitute for $\Gamma(x+1)$ and $\Gamma(x+\Delta x+1)$ their respective algebraical approximate values

$$(2\pi x)^{\frac{1}{2}} \left(\frac{x}{e}\right)^x \text{ and } (2\pi(x+\Delta x))^{\frac{1}{2}} \left(\frac{x+\Delta x}{e}\right)^{x+\Delta x};$$

extract the x th root of the first equation and the $(x+\Delta x)$ th root of the second; and divide the latter by the former, remembering that the exponent of p_{y+1} is in fact merely $\frac{1}{p_{y+1}}$ or $\frac{1}{x+\Delta x}$; and we get

$$(x+\Delta x)^{\frac{1}{x+\Delta x}} = \left(1 + \frac{\Delta x}{x}\right) (2\pi(x+\Delta x))^{\frac{1}{2(x+\Delta x)}} (2\pi x)^{-\frac{1}{2x}};$$

an equation for determining Δx in terms of x .

This equation becomes readily soluble if we avail ourselves of an observed fact with reference to prime numbers, that Δx is a small quantity as compared with x itself, at least in that part of the series of ordinals which is at some distance from the commencement. Taking then the logarithms of both sides of this equation, and using the approximate expressions

$$\log(x+\Delta x) = \log x + \frac{\Delta x}{x},$$

and

$$\frac{1}{x+\Delta x} - \frac{1}{x} = -\frac{\Delta x}{x^2},$$

we have

$$\Delta x = \log x + \frac{\Delta x}{2x} \left(1 + \log \frac{2\pi}{x}\right) + \frac{(\Delta x)^2}{2x}.$$

The approximate solution of this equation obtained by rejecting the terms divided by x is

$$\Delta x = \log x;$$

and a more exact solution may be obtained by substituting $\log x$ for Δx in the terms previously rejected; which gives

$$\Delta x = \log x \left(1 + \frac{1}{2x} \log(2\pi e)\right);$$

but the first solution is all that will ordinarily be required.

Now y being the number of primes up to x , if $x = \phi y$, we have

$$x + \Delta x = \phi(y+1) = \phi y + \phi'y$$

nearly; and therefore

$$\Delta x \text{ or } \log x = \phi'y = \frac{dx}{dy};$$

and finally,

$$y = \int \frac{dx}{\log x} + c,$$

or the number of primes between the limits x and x' is $\text{lix}' - \text{lix}$. If we take the more accurate value of Δx , as deduced by the above method, and make

$$x + \Delta x = \phi y + \phi' y + \frac{1}{2} \phi'' y,$$

and use the approximate value already obtained for determining the last term, we shall get

$$y = \text{lix} - \frac{1}{2} \log(2\pi)(\log \log x) + c;$$

so that the correction is scarcely appreciable.

In applying this formula, we must bear in mind, that as it is a continuous algebraical expression derived from the application of a principle of means, it represents not the absolute number of primes which will actually be found between any particular limits, but the average number which may be expected to occur within such limits, having regard to the general law under which primes occur throughout the ordinal series. The expression $\log x$ represents nearly *the average distance* between two primes at the point x in the ordinal series; and with reference to that part of the ordinals which lies near this point, that is, so near that $\log x$ has undergone no appreciable change, it may be expected to indicate with tolerable accuracy the average distance which a table of primes would show. The larger the range over which we traverse, the more nearly shall we be able to test the formula, or ascertain the nature and extent of its deviation from truth; and the largeness of the range must be estimated with regard to the actual magnitude of the limits.

The formula for determining the difference between two logarithm-integrals is a series of powers of t , where t denotes the logarithm of the ratio of the limits; the series being convergent with considerable rapidity when t is less than one.

If the lower limit be x , the upper limit x' , and $\log \frac{x'}{x} = t$, and

$\frac{1}{\log x} = v$, we have

$$\begin{aligned} \text{lix}' - \text{lix} = & (x' - x)v - \frac{1}{2} t^2 v^2 x \left\{ 1 + \frac{2 - 2.1v}{3} t \right. \\ & \left. + \frac{3 - 3.2v + 3.2.1v^2}{3.4} t^2 + \frac{4 - 4.3v + 4.3.2v^2 - 4.3.2.1v^3}{3.4.5} t^3 + \dots \right\}, \end{aligned}$$

of which a very few terms will suffice for our purpose.

The following Table was constructed, for convenience of computation, with the value $t = \frac{1}{2}$; so that the numbers proceed in a geometrical series whose common ratio is $e^{\frac{1}{2}}$. This proceeds regularly from 100 to 180,804; the limits following this were selected in order to compare the results with the number of primes counted from the tables by Legendre:—

Lower limit.	Upper limit.	Primes computed.	Primes counted.	Difference.
100	165	13.3	13	.3
165	272	20.0	20	.0
272	448	29.7	28	1.7
448	739	45.4	45	.4
739	1218	69.6	68	1.6
1218	2009	107.1	105	2.1
2009	3312	165.3	161	4.3
3312	5460	256.5	256	.5
5460	9002	399.0	397	2.0
9002	14841	622.5	620	2.5
14841	24469	975.5	975	.5
24469	40343	1530.0	1517	13.0
40343	66514	2406.6	2398	8.6
66514	109663	3793.5	3794	-.5
109663	180804	5992.6	5982	10.6
180804	350000	13569.7	13572	-2.3
350000	400000	3896.0	3884	12.0
400000	700000	22722.5	22674	48.5
700000	1000000	21983.5	21958	25.5
1000000	2000000	70430.0		
2000000	3000000	67916.0		
3000000	4000000	66381.8		

It will be seen from this Table, that at the commencement of the numeral series, the formula is in excess to the extent of about 1 in 600; but from the character of the investigation, I am disposed to think that this error diminishes as we advance in the series. On the whole the coincidence is of a remarkable character, having regard to the nature of the subject; for judging merely from the apparently irregular occurrence of primes, it might be thought impossible that their law should be represented in any sense by a continuous analytical formula.

Assuming the logarithm-integral to represent the number of primes between its limits, it is easy to see how it becomes possible to frame a formula like that of Legendre's $\left(\frac{x}{\log x - 1.08366}\right)$, which will give approximately true results, at least over a small range of the series. The variation of the logarithm is so slow as compared with that of the number, that it may be treated as constant within certain limits; and the numerical correction becomes necessary in conse-

quence of the computation being made from the commencement of the series, where the variation of the logarithm is greatest.

With a view of applying the formula in somewhat higher parts of the series, I have computed the primes from 2,010,000 to 2,019,000 at 620, while Burckhardt's Tables give 617; and from 2,982,000 to 3,018,000 at 2,413, which agrees exactly with the Tables; but these ranges are too limited to enable us to judge of the formula.

Since the preceding table was computed, I have found the following formula, which gives the most accurate and expeditious method of calculating the logarithm-integrals of large numbers :

$$\text{lix}' - \text{lix} = (x' - x)v - xt^2v^2\{D - tvD + t^2v^2D^2 - \dots\} \left(\frac{\varepsilon^t - 1}{t}\right),$$

where D denotes differentiation with regard to t .

When $t=1$, this becomes

$$(x' - x)v - xv^2\{1 - v(\varepsilon - 2) + v^2(6 - 2\varepsilon) - v^3(9\varepsilon - 24) + v^4(120 - 44\varepsilon) - v^5(265\varepsilon - 720) + \dots\};$$

the general term of the part within brackets abstracted from its sign being

$$\pm v^{n-1}(1.2.3\dots n) \left(1 - \varepsilon \left(1 - \frac{1}{2} + \frac{1}{2.3} - \dots \pm \frac{1}{2.3\dots n}\right)\right),$$

or

$$v^{n-1}\varepsilon \left(\frac{1}{n+1} - \frac{1}{(n+1)(n+2)} + \frac{1}{(n+1)(n+2)(n+3)} - \dots\right),$$

which diminishes as n increases, and is always positive. In the form

$$\text{li}\varepsilon^{m+1} - \text{li}\varepsilon^m = \frac{\varepsilon^{m+1} - \varepsilon^m}{m} - \frac{\varepsilon^m}{m^2} \left(1 - \frac{.7182818}{m} + \frac{.5634364}{m^2} - \frac{.4645365}{m^3} + \frac{.3955996}{m^4} - \dots\right)$$

it can be calculated with great ease, particularly when m is an integer. The following are instances :

Primes from 300000 to 815484 ; computed	39089 ;	counted	39082
... 183939 to 500000 ;	... 24890 ;	... 24883	
... 331091 to 900000 ;	... 42819 ;	... 42778*.	

The circumstance that $\log x$ represents the average distance between two primes at the point x in the ordinal series, gives

* I subjoin the following Table of logarithm-integrals, which will serve

us a perfect idea of the rate at which primes occur. Thus if we wish to know at what point of the ordinal series the primes come at the rate of 50 per thousand, the answer is $\epsilon^{\frac{1000}{50}}$ or ϵ^{20} , which is about 485,000,000; that is, for many millions before and after 485 millions, the rate of primes is 50 to the thousand.

For ranges of moderate magnitude, the formula

$$\frac{(x' - x)^2}{x'(\log x' - 1) - x(\log x - 1)}$$

will be found to represent the number of primes between x' and x with considerable accuracy in any part of the series. For since $\log x$ is the average distance between two primes at x , the average of these average distances from x to x' will be

$$\frac{x'(\log x' - 1) - x(\log x - 1)}{x' - x};$$

and if $x' - x$ be divided by this expression, we obtain the formula above written as the average number of primes between x' and x . This would give us for the primes between x and $2x$ the expression

$$\frac{x}{\log x + 2 \log 2 - 1} \quad \text{or} \quad \frac{x}{\log x + \cdot 38629436};$$

which will be found to be nearly correct. The above formula

for any number up to about 1200,000,000 by one application of the formula for $\text{lix}' - \text{lix}$.

x .	$\log x$.	Lix .
2·7182818	1	1·8951178
7·389057	2	4·9542360
20·08553	3	9·933834
54·59815	4	19·63029
148·4133	5	40·18532
403·4288	6	85·98970
1096·6333	7	191·3349
2980·958	8	440·2102
8103·084	9	1037·7084
22026·466	10	2487·8509
59874·14	11	6067·0276
162754·78	12	14955·153
442413·36	13	37193·320
1202604·3	14	93188·15
3269017·8	15	234237·22
8886110·0	16	594842·2
24154949·4	17	1523419·2
65659972·	18	3884686·0
178482268·	19	9957687·
485165153·	20	25622432·

appears in some cases to give results more closely corresponding with the Tables than are derived from the logarithm-integral. Thus for the primes from 100,000 to 1,000,000 it gives 68,853, the actual number being 68,901.

Prop. 4. To find the sum of any function (ϕ) of the primes up to x , the n th prime.

If this sum (exclusive of the function of x itself) be denoted by ψn , we have (x being a function of n , say μn)

$$\phi(2) + \phi(3) + \phi(5) + \dots + \phi(\mu(n-1)) = \psi n,$$

$$\phi(2) + \phi(3) + \phi(5) + \dots + \phi(\mu(n-1)) + \phi(\mu(n)) = \psi(n+1);$$

whence

$$\Delta \psi n = \phi x,$$

and

$$\psi n = \sum \phi x = c + \int \phi x dn - \frac{1}{2} \phi x + \frac{1}{6} \cdot \frac{1}{2} \frac{d(\phi x)}{dn} - \frac{1}{30} \frac{1}{2 \cdot 3 \cdot 4} \frac{d^3(\phi x)}{dn^3} + \dots$$

Now by the last proposition

$$n = \text{lix} \text{ or } dn = \frac{dx}{\log x};$$

whence

$$\psi n = c + \int \frac{\phi x}{\log x} dx - \frac{1}{2} \phi x + \frac{1}{12} \phi' x \cdot \log x - \frac{1}{720}$$

$$\left(\phi''' x \cdot (\log x)^3 + 3 \phi'' x \frac{(\log x)^2}{x} + \phi' x \frac{1}{x^2} (\log x - (\log x)^2) \right) + \dots$$

Cases of the above Theorem.

Case 1. Let $\phi x = x$; then the sum of the primes up to x inclusive is

$$c + \int \frac{x dx}{\log x} + \frac{1}{2} x + \frac{1}{12} \log x + \frac{1}{720} \frac{1}{x^2} ((\log x)^2 - \log x) - \dots;$$

or

$$c + \text{li}(x^2) + \frac{1}{2} x + \frac{1}{12} \log x + \dots;$$

that is, the *sum* of the primes from y exclusive to x inclusive, corrected by deducting half the difference between y and x , (for the other terms may be neglected), is equal to the *number* of primes between y^2 and x^2 .

This remarkable theorem, which connects in an unexpected manner the *values* of the primes in one part of the series with their *number* in another and remote part of the series, will enable us to verify the theorem as to the number of primes, and will confirm the conclusion that the relative error in determining the number of primes diminishes as the numbers

increase in magnitude. In determining (by the formula $\text{li } 2000 - \text{li } 1000$) the number of primes between 1000 and 2000, there would be an error in excess of about 2; so that if we sum the *actual* primes between 1000 and 2000, in order to obtain the number of primes between 1,000,000 and 4,000,000, we might expect to obtain a number varying from the theoretical number of primes between these limits by from 2000 to 4000; but as it will be seen that the error is not so great, and that it is not permanently in defect, we may infer that the formula for computing the number of primes is more correct for large numbers.

The sum of the actual primes between 1000 and 2000 is 205,054, which leaves, after deducting $\frac{1}{2}(1999-997)$, the number 204,553; and the computed number of primes between 997^2 or 994,009 and 1999^2 or 3,996,001 is about 204,900. If we take in another prime, the difference lies in the other direction; for the former number becomes 206,554, and the latter about 205,908.

To take an instance in which the number of primes has been counted, we find the sum of the primes from 241 exclusive to 503 inclusive (with the proper correction) to be 16,219, and the number of primes from 241^2 or 58,081 to 503^2 or 253,009 to be 16,326; and taking the next prime, the numbers are respectively 16,727 and 16,861. If we had begun one prime earlier, the error would have lain in the other direction. These examples sufficiently illustrate the general character of the theorem.

Case 2. Let $\phi x = x^2$; then the sum of the squares of the primes up to x inclusive is

$$c + \text{li}(x^3) + \frac{1}{2}x^2 + \frac{1}{6}x \log x - \frac{1}{720} \frac{1}{x} (4(\log x)^2 + 2 \log x) + \dots;$$

or the sum of the squares of the primes from y exclusive to x inclusive is

$$\text{li}(x^3) - \text{li}(y^3) + \frac{1}{2}(x^2 - y^2) + \frac{1}{6}(x \log x - y \log y) \text{ nearly.}$$

Generally it will be found that the sum of the $(m-1)$ th powers of the primes from y exclusive to x inclusive is the number of the primes between y^m and x^m increased by $\frac{1}{2}(x^{m-1} - y^{m-1})$, with further corrections involving the $(m-2)$ th and lower powers of y and x in combination with their logarithms, which would be of small relative amount.

By giving to m fractional values, we shall find that the sum

of the square roots of the primes from y exclusive to x inclusive, diminished by $\frac{1}{2}(\sqrt{x} - \sqrt{y})$, is the number of the primes from $y^{\frac{3}{2}}$ to $x^{\frac{3}{2}}$; and a similar theorem may be expressed for any other roots, or for roots of powers.

As an example, the sum of the square roots of the primes from 1789 exclusive to 2399 inclusive is 3638.5; from which we deduct 3.4 for half the difference of the roots, leaving 3635; and it will be found by counting, that the number of primes from $1789^{\frac{3}{2}}$ or 75,668 to $2399^{\frac{3}{2}}$ or 117,502 is 3631; a degree of accuracy which arises from the circumstance that between 1800 and 2400 there is no great excess or defect of primes from the average number.

The sum of the cube roots of the primes between the same limits, duly corrected, is 1013; and the number of primes between $1789^{\frac{4}{3}}$ or 21,717 and $2399^{\frac{4}{3}}$ or 32,115 is 1008.

Case 3. Let $\phi x = \frac{1}{x}$; then the sum of the reciprocals of the primes up to x inclusive is

$$c + \int \frac{dx}{x \log x} + \frac{1}{2x} - \frac{1}{12} \frac{\log x}{x^2} - \dots$$

or

$$c + \log \log x + \frac{1}{2x} \text{ nearly.}$$

The sum of the reciprocals of the primes, therefore, from y exclusive to x inclusive, is

$$\log \frac{\log x}{\log y} + \frac{1}{2} \left(\frac{1}{x} - \frac{1}{y} \right).$$

It follows from this that the sum of the reciprocals of the primes from any number to the prime next to its n th power is nearly $\log n$, whatever the number may be; the corresponding proposition for ordinals being that the sum of the reciprocals from x to nx is nearly $\log n$, and is independent of the value of x . We also see that the sum of the reciprocals of the primes up to a large number x differs only by a constant from the sum of the reciprocals of the ordinals up to $\log x$.

The constant c would require, like the corresponding constant of the ordinal series (γ or .5771213), to be determined by computation. By employing values of x up to about 900, the constant appears to be nearly .267; but as at the commencement of the series the actual primes differ sensibly from what may be called the theoretical primes, it is difficult to determine the constant thus with accuracy.

Case 4. Let $\phi x = \frac{1}{x^2}$; then the sum of the inverse squares of the primes up to x inclusive is

$$c + \int \frac{dx}{x^2 \log x} + \frac{1}{2x^2} - \frac{1}{6} \frac{\log x}{x^3} - \dots$$

or

$$c + \text{li}\left(\frac{1}{x}\right) + \frac{1}{2x^2} \text{ nearly.}$$

The value of c is the sum of the series *ad infinitum*, for all the other terms vanish when x is infinite. It was computed by Euler at .4542247.

Generally, we shall find that the sum of the inverse n th powers of the primes up to x inclusive is nearly

$$c + \text{li}\left(\frac{1}{x^{n-1}}\right) + \frac{1}{2x^n},$$

where c is the sum *ad infinitum*; and similar formulæ apply to negative fractional powers. The logarithm-integrals may be found by means of Soldner's table and formulæ.

Case 5. Let $\phi x = \log x$; then the sum of the logarithms of the primes up to x inclusive is

$$c + \int dx + \frac{1}{2} \log x + \frac{1}{12} \frac{\log x}{x} - \dots$$

or

$$c + x + \frac{1}{2} \log x \text{ nearly.}$$

This theorem, which imports that the sum of the logarithms of the primes from y exclusive to x inclusive is nearly $x - y$ (more accurately $x - y + \frac{1}{2} \log \frac{x}{y}$), may be verified by trial, and will be found to be true of large numbers in an average sense. It is true in the same sense as the formula for computing the number of primes, and is in effect identical with it. If we take limits between which there are more than the average number of primes, the sum of the logarithms will be too large, and *vice versa*; but as x increases, the sum of the logarithms of the primes up to x becomes equal to x .

Case 6. If $\phi x = \log \left(1 - \frac{1}{x}\right)^{-1}$, we shall find approximately

$$\begin{aligned} & \log \left\{ \left(1 - \frac{1}{2}\right)^{-1} \left(1 - \frac{1}{3}\right)^{-1} \left(1 - \frac{1}{5}\right)^{-1} \dots \left(1 - \frac{1}{x}\right)^{-1} \right\} \\ &= c + \log \log x + \frac{1}{2} \text{li} \left(\frac{1}{x}\right) + \frac{1}{3} \text{li} \left(\frac{1}{x^2}\right) + \frac{1}{4} \text{li} \left(\frac{1}{x^3}\right) + \dots \\ & \quad + \frac{1}{2} \log \left(1 - \frac{1}{x}\right)^{-1}, \end{aligned}$$

which, as x increases without limit, approaches to $c + \log \log x$ or $\log \log (x^\kappa)$, where κ or ϵ^c is a constant, which may be found by computation.

Now as x increases without limit, we have

$$\log \left(1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \dots + \frac{1}{x^\kappa} \right) = \log \log (x^\kappa);$$

so that the expression to which $\left(1 - \frac{1}{2}\right)^{-1} \left(1 - \frac{1}{3}\right)^{-1} \left(1 - \frac{1}{5}\right)^{-1} \dots \left(1 - \frac{1}{x}\right)^{-1}$ approximates arithmetically is not $1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \dots + \frac{1}{x}$, as was before observed, but $1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \dots + \frac{1}{x^\kappa}$.

The expression here discussed was calculated by Legendre, and tabulated up to $x=1229$. (See *Théorie des Nombres*, tab. IX. vol. i.) The value of c , as derived from $x=1229$, is $\cdot 581078$; as derived from $x=1213$, it is $\cdot 580693$; and as derived from $x=947$, it is $\cdot 58101$; and it is not improbable that its theoretical value is the constant $\cdot 5771213$ of the ordinal series. It may be observed, that the difference between this constant and that which enters in the sum of the reciprocals of the primes corresponds exactly with the analytical value of this constant γ ; for the value of this difference is

$$\left(\frac{1}{2} + \log \frac{1}{2}\right) + \left(\frac{1}{3} + \log \frac{2}{3}\right) + \left(\frac{1}{5} + \log \frac{4}{5}\right) + \dots \text{ad inf.},$$

and the value of γ is

$$(1 + \log 1) + \left(\frac{1}{2} + \log \frac{1}{2}\right) + \left(\frac{1}{3} + \log \frac{2}{3}\right) + \left(\frac{1}{4} + \log \frac{3}{4}\right) + \dots \text{ad infinitum}.$$

The preceding investigations may be made available for the determination of the law which regulates the occurrence of numbers which are composites of two primes, of three primes, &c.; but I have not hitherto investigated this part of the subject with sufficient minuteness.

VI. On the Development of Electricity in the Act of Muscular Contraction. By M. BECQUEREL*.

I HAVE repeated unsuccessfully the experiment of M. Du Bois Reymond, relative to the production of an electric current in the act of muscular contraction, making use of the arrangements which he indicated in a letter addressed to

* From the *Comptes Rendus* for May 28, 1849.

M. Arago by M. de Humboldt, dated the 17th of May*, excluding however all those secondary causes which could give rise to electric currents, excepting that one the action of which is described.

I shall commence by recalling to mind the observations which I made in studying the electric effects obtained with a condenser, the plates of which were made of platinum or copper gilt (*Traité de l'Electricité et du Magnétisme*, t. v. 2^e partie, page 10):—

“The electro-chemical effects produced on the contact of acid solutions with the liquids which moisten the fingers, must be taken into account. In these various reactions the acids acquire the positive electricity, which is transmitted to the plate, and the liquids which moisten the fingers the negative electricity. With the alkalies the effects are inverse.”

It follows from this, that if one of the plates is covered externally by a very thin layer of hygrometric water, and that it is touched with a finger moistened with perspiration, electric effects, resulting from the reaction of the perspiration upon the water, ensue. It is also produced when a finger in a great state of transpiration is applied upon one of the plates, after having been previously moistened with water; in this case the water acquires the positive electricity, and the contrary electricity flows into the body of the experimenter. If we add to these effects those which take place when foreign bodies are adherent to the skin, we must conceive that a large number of complex electric effects would be produced in plunging two fingers, as is done by M. Du Bois Reymond, into two capsules filled with water in which are contained two plates of platinum in communication with a multiplier. This is not all: when, in virtue of these various causes, a current has circulated in the liquid and in the wire, the two plates of platinum are polarized in opposite directions, as may be shown by withdrawing the fingers and establishing the communication between the two capsules by means of a siphon filled with the same liquid as that which they contain. This current, during the first few moments, having the same intensity as the primitive current, annuls it; but if, in the act of contraction, the finger of the contracted hand become more or less immersed in water, the inverse current may be less or superior to the direct current. I guarded not only against the effects of the inverse current, but also against the effects resulting from the greater or less immersion of the fingers by smearing with fat those parts of the fingers which might temporarily come into contact with the liquid. By proceeding in this manner, I found it impossible to observe the effects described by M. Du Bois Reymond.

* See Phil. Mag. vol. xxxiv. p. 543.

VII. *Note relative to the Electricity developed by Muscular Contraction.* By M. C. DESPRETZ*.

THE note which I have the honour of communicating, is a simple enumeration of the experiments which I made with the view of reproducing the phænomena announced by M. Du Bois Reymond of Berlin†. I shall not discuss these phænomena, my only object being to reproduce them. This appears to me the most philosophical manner of proceeding in the appreciation of a new fact. We are not sufficiently acquainted with the intimate nature of bodies, of heat, light or electricity, we know too little regarding vital phænomena, to reject *à priori* a physico-physiological fact, however singular it may appear at first sight. The wisest and most prudent course is, first to confirm it, and afterwards by a profound analysis of the phænomenon, to ascertain the various sources of error to which it is exposed. It must not be forgotten that Galvani's researches upon animal electricity have given rise to one of the most important discoveries of modern times.

I shall not enter further upon the history of the subject, in doing which I should have to quote numerous researches, and particularly those of the illustrious philosopher who made known to the Academy the principal result obtained by M. Reymond, and whose name is connected with so many important researches which have been made since the end of the last century.

I have not inserted in the *Comptes Rendus* the results of the experiments which I alluded to at the last meeting of the Academy, because these results did not appear to me sufficiently demonstrative. The only conclusion I should deduce from my first experiments, would be the uncertainty which the intervention of the metallic plates in galvanometers appears to me to throw upon the results of many experiments, as I have had the honour of stating to the Academy.

The galvanometer which I used was made by M. Ruhmkorff, whose skill is well known. The diameter of the wire was $\frac{1}{10}$ th of a millimetre, and its length 300 metres. The wire made about 1800 convolutions round the frame of the apparatus. The delicacy of the instrument is shown by the following numbers.

A copper wire $\frac{3}{4}$ ths of a millimetre in diameter, when immersed to a depth of 2 centimetres, afforded a deflection of 3° in distilled water, 25° in the water of the river Seine, and 68° in a solution of chloride of sodium, containing from four to five per cent. of the salt.

Plates of gold, the surfaces of which were nearly one square

* From the *Comptes Rendus* for May 28, 1849.

† *Comptes Rendus*, May 21; and *Phil. Mag.* vol. xxxiv. p. 543.

centimetre, afforded under the same circumstances deflections of 11° , 24° and 85° . The gold, which was perfectly pure, had been recently prepared at the Mint of Paris, in the laboratory of M. Pelouze and M. Peligot. The needle, when set free, took about half a minute in moving from 50° to zero.

I was not at first acquainted with M. Raymond's method of proceeding. In my earliest experiments, two cylindrical conductors were held in the hands, and when the needle had returned to zero, or had acquired a perfectly stationary position, one of the arms was powerfully contracted; the deflection was then observed, and when the needle had again become stationary after the cessation of the contraction, the other arm was strongly contracted, also observing the deflection.

These first experiments were made with common copper conductors. But for the sake of avoiding the objection which might arise from the ready oxidation of this metal, I had them covered with gold-leaf. Other conductors were coated with silver, platinum and gold.

The experiments were made by three persons. Were there no occasion for using galvanometers, we should be inclined to believe that silver, and especially gold and platinum, which preserve their polish and lustre in contact with moist air, would be appropriate for these experiments, on account of their unalterability. This is however not the case; silver, gold and platinum afford currents which are almost as strong as copper. When the platinum conductor is held in the hand, and the needle has become stationary, merely touching it with one finger more, or less, is sufficient to change the position of the needle several degrees.

In these experiments the needle was deflected 50° , 75° and even 90° . When one of the conductors was squeezed powerfully, the needle moved in one direction, and when the other conductor was squeezed, the needle moved in the same or in an opposite direction.

It is indispensable to repeat these experiments several times, without which we should be liable to errors. Thus it happens that the deflections of the needle occur alternately in one and the other direction; but on multiplying the experiments, we find that the deflections frequently take place in the same direction, although the compression is produced first by one and then by the other arm. If the chemical action was regular like that of a watch-spring, we ought to obtain currents in the opposite direction. We only experimented in this way, because, on the one hand, we were unacquainted with M. Raymond's method of proceeding; and on the other, we thought that silver, and especially gold and platinum, when simply held in the hand without any compression, would only afford

a very weak current. But experience unfortunately proves that gold and platinum are under these circumstances as *impressionable* as brass, if I may be allowed to use such an expression. I have repeated M. Reymond's experiment several times, both following rigidly and varying his method of proceeding.

I first wished to ascertain whether the instrument, which I had not yet used, was sensible or not to changes of temperature. For this purpose, I heated one of the places at which it was soldered to the melting-point of wax, the communication being established by the hands between the two plates; I also augmented the temperature of one of the two solutions of common salt, by immersing in it glass tubes filled with boiling water, the communication being always kept up by the hands; in neither case did I observe the slightest deflection, which might be anticipated from the known properties of thermo-electric phenomena; nevertheless it appeared to me of use to verify this in the present instance. To avoid the effect of a more or less deep immersion of the metallic plates, in consequence of the introduction of the fingers, I partly covered these plates with black wax, so that the uncovered surface was always in contact with the solution.

As regards the fingers, I attempted to immerse them to the same extent in all the experiments, having found that in plunging successively one, two or three, or more fingers, or a single finger to a greater or less depth, the intensity of the deflections varied. This result indeed had been anticipated. I had even had some long kinds of copper thimbles gilt, so as to regulate the immersion better. But I abandoned this method of proceeding, because it differed too much from that adopted by M. Du Bois Reymond.

In experiments made according to M. Du Bois Reymond's process, the alternate contraction of each arm has sometimes afforded deflections in the same, sometimes in the opposite direction.

In other experiments, each arm was successively contracted out of the water, and on each contraction the vessels were connected by means of the fingers. In others, large capsules were used, so as to allow more freedom of motion of the hands, and so as to permit the immersion of the closed hands, either contracted or not contracted. The results of these two series of experiments are sometimes favourable, sometimes contrary to the assertion of M. Reymond. The necessity of multiplying the experiments is very distinctly shown in this case. The results of two or three experiments agree with the results announced by M. Reymond; and then, if they are continued, opposite results are obtained. A singular fact is also remarked

in these experiments, viz. that the fingers are influenced much in the same way as metallic conductors; they lose part of their efficacy by repeated immersions.

I was desirous of reducing the experiment to a greater amount of simplicity. I replaced the galvanometer by a frog which was properly prepared. Several persons, separately or in connection, having strongly contracted one of their arms, in vain endeavoured to produce convulsive movements, by connecting the two arms by means of the most sensible parts of the animal. Nevertheless with a very fine copper wire and a plate of zinc, without the use of any liquid, very marked contractions were produced both before and after the experiment.

I also endeavoured in vain to deflect a very delicate astatic magnetic needle, by the union of the two hands, whilst one hand was strongly contracted. Finally, I attached a cylindrical gilt conductor to the back of each hand by silk cord: the contraction of one or the other arm did not perceptibly change the deflection of the needle, which amounted to 10° from the simple contact. The effects of the contact were increased in a marked degree by moistening the back of the hand with a few drops of salt water; but the contraction of one or the other arm did not produce deflections alternately in one or the other directions.

These three experiments appear to me to be under more favourable conditions than those of M. Reymond. The results of them are removed from the intervention of the immersion of metallic laminæ in saline solutions, which is always somewhat obscure. Unfortunately they only furnished negative results.

In conclusion, if we are only to admit as true that which is clearly demonstrated, we think that the experiments detailed in this note show, that if the contraction of one arm gives rise to an electric current, this current is not appreciable to our present means, at least to those which we have employed.

We are however far from believing that the tetanic contraction of a limb does not give rise to the decomposition of a certain quantity of electricity. The friction of the parts upon each other, and the unequally heated state of heterogeneous parts would give rise to electric decompositions; but recompositions ensue immediately. This is probably the case in all the chemical actions which occur in the œconomy.

Until chemistry has discovered a metal or an alloy which does not afford any current by the contact of liquid conductors, we shall always be exposed to numerous errors in researches upon the currents of animals and vegetables.

The galvanometer is a very valuable instrument, but it requires a very large amount of skill and prudence on the part

of the experimenter. If it is made but slightly sensible, it only indicates powerful phenomena; if it is made very delicate, it obeys the slightest perturbing causes. It is not impossible that a large number of experiments upon the currents in animals and vegetables may merely arise from illusions, and that what is attributed to animal and vegetable currents, may be nothing more than the action of liquids upon the plates of gold or platinum of galvanoscopes, or upon other different liquids. If the two plates of gold of a galvanoscope are inserted in any direction in a potato which has either budded or not, in an apple, or a cabbage-stalk, or the flesh of beef; if any two parts of the skin, slightly moist, are touched with these same plates, we have currents; if first one and then the other plate be withdrawn in succession, and after having washed and wiped it, it be replaced, the current is reversed; if the plates are more or less deeply immersed, reversions may also occur.

It is possible that the convulsions experienced by the frog from the contact of the crural nerves with the muscles of the legs, may depend only upon the heterogeneity of the liquids which moisten these parts. It is possible that the permanence of the direction of what is called the current of the frog may be owing to a different alterability of the extremities of the animal by the various solutions employed in these experiments. In the experiment as arranged for determining the true or false current in the frog, we merely require to substitute for the animal a cord of thread impregnated with common salt, and one of the ends of which has been touched with the stopper of a bottle of sulphuric acid, and the other with the stopper of a bottle of nitric acid, to reverse the current a great many times, as is done in the case of the current of the frog.

There is one experiment upon this subject which would have a certain value without being decisive, it is that of the action of a circuit of frogs upon a magnetic needle.

I arranged a chain of frogs in the same manner as the pairs of a voltaic pile are arranged; this chain traversed a bell-glass, beneath which a very delicate astatic needle was suspended. I did not observe any distinctly-appreciable effect at the moment at which I united or separated the extremities of the chain. Had an effect been obtained, the objection of the action of the heterogeneous moist parts would still remain.

It does not appear to me that the existence of electric currents in frogs and plants is a perfectly proved fact. I speak openly, submitting my doubts to those philosophers who have made most interesting, and in some cases very ingenious experiments upon this subject.

VIII. *Some facts relative to the Spheroidal State of Bodies, Fire-Ordeal, Incombustible Man, &c.* By P. H. BOUTIGNY (d'Evreux)*.

IN the year 241, Sapor or Chapour ordered the Magi to do all in their power to persuade them and bring them back to the faith of their ancestors. It was then that one of the pontiffs of the dominant religion, Adurabâd-Mabrasphand, offered to submit to the fiery ordeal . . . "He proposed that eighteen pounds of melted copper, issuing from the furnace, all hot, should be poured on his naked body, on condition that, if he was not injured by it, the unbelievers should yield to so great a miracle. The trial was said to be attended with such success, that they were all converted." The historian adds, with an air of doubt, certainly allowable in such a matter, "We see that the religion of Zoroastre had also its miracles and its legends†."

Now this fiery ordeal, undergone with such success by Adurabâd-Mabrasphand, is in plain truth an experiment of primitive facility and simplicity, and which is anything but miraculous.

I stop here an instant, for I fancy that I see the smile of incredulity rise on the lips of some who do me the honour of listening to me;—that smile, so discouraging to one who is insincere, but which only heightens the ardour of him who intends to practise no deception, and who does all in his power not to deceive himself.

To such persons then I would offer this encouragement; the little that I have still to relate appears improbable, but it is true, and that is enough. Having said this, I continue.

In France, in England, in Italy, wherever I have had occasion to speak of bodies in the spheroidal state, I have met with persons who have put to me this question: May there not be some connection between these phænomena and that presented by men who run barefooted over liquid metal (?) still incandescent, or who plunge their hand into molten lead, &c.‡? To all I have answered, Yes, I believe that there is an intimate relation between all these facts and the spheroidal state. And then, in my turn, I put this question: Have you witnessed the fact which you tell me? And the answer has invariably been in the negative.

I avow that all these *on-dits* and the marvellous legends

* From the *Comptes Rendus* for May 14, 1849.

† *Dictionnaire historique, critique et bibliographique*, t. xxvii. p. 417.

‡ I have alluded to these facts in the work entitled, *Nouvelle branche de Physique, or Etudes sur les Corps à l'Etat sphéroïdal*, p. 36.

which I had read in various works* on the fiery ordeal and incombustible men, admitted without reserve by some, obstinately denied by others, excited my curiosity greatly, and gave me a great desire to verify all these phænomena, and to recall them to the recollection of contemporary observers; for, alas! all this is as old as the world; *nil sub sole novum*.

I wrote first to my friend Dr. Roché, who passes his life in the midst of the blast furnaces of the Eure, and who is the physician of a portion of the Cyclopean population who feed them. I requested of him precise particulars. All that he could ascertain was, that a man named La Forge, of from thirty-five to thirty-six years of age, very corpulent, walked step by step barefooted on the pigs after the casting: but he had not seen this. This was not enough to dispel my doubts.

I then applied to a foundry at Paris, where I was laughed at and shown the door. I retired, hanging down my ears, thinking over the difficulties of verifying a single fact, and such a simple one.

Subsequently I was fortunate enough to meet with M. Alph. Michel, who lives in the midst of the forges of Franche-Comté. M. Michel promised me, with the greatest kindness, to inquire into these facts, and to report upon them if desired.

The following is an extract from the letter which he did me the honour to write to me, dated the 26th of last March:—

“On my return home, I did not fail to obtain information from the workmen of the facts of the case (the immersion of the finger in the incandescent melted metal), and most of them laughed in my face, which did not deter me. Lastly, being one day at the forge of Magny, near Lure, I put the question again to a workman, who answered that nothing was more simple; and, to prove it, at the moment when the metal in a state of fusion issued from a Wilkinson, he passed his finger into the incandescent jet. A person employed in the establishment repeated the experiment with impunity: and I myself, emboldened by what I saw, did the same. . . . I may observe, that, in making this trial, none of us moistened his finger.

“I hasten, Sir, to acquaint you with this fact, which seems to support your ideas on the globular state of liquids; for the fingers being naturally more or less humid, it is, I think, to this moisture passing to the spheroidal state, that we must ascribe their momentary incombustibility.”

The following are the experiments which I have made:—

I divided or cut with my hand a jet of melted metal of five

* *Des Erreurs et des Préjugés répandus dans les diverses classes de la Société*, t. xii. p. 183.

to six centimetres, which escaped by the tap, then I immediately plunged the other hand in a pot filled with incandescent metal, which was truly frightful to look at. I involuntarily shuddered. But both hands came out of the ordeal victorious. And now, if any thing astonishes me, it is that such experiments are not quite common.

I shall of course be asked, what precautions are necessary to preserve oneself from the disorganizing action of the incandescent matter? I answer, None;—only to have no fear, to make the experiment with confidence, to pass the hand rapidly, but not too rapidly, in the metal in full fusion.

Otherwise, if the experiment were performed with fear, or with too great rapidity, the repulsive force might be overcome which exists in incandescent bodies, and thus the contact with the skin be effected, which would undoubtedly remain in a state easy to understand.

To form a conception of the danger there would be in passing the hand too rapidly in the metal in fusion, it will suffice to recollect that the resistance is proportionate to the square of the velocity, and, in so compact a fluid as liquid iron, this resistance increases certainly in a higher ratio.

The experiment succeeds especially when the skin is humid; and the involuntary dread which one feels at facing these masses of fire, almost always puts the body into that state of moisture so necessary to success; but by taking some precautions, one becomes veritably invulnerable. The following is what has succeeded best with me: I rub my hands with soap, so as to give them a polished surface; then, at the moment of making the experiment, I dip my hand into a cold solution of sal-ammoniac saturated with sulphurous acid, or simply into water containing some sal-ammoniac, and, in default of that, into fresh water.

Regnault, who has occupied himself with this subject, says, "Those who make a trade of fire handling and holding it in the mouth, sometimes employ an equal mixture of spirit of sulphur, of sal-ammoniac, of essence of rosemary, and onion-juice." All volatile substances, we see, which, in evaporating, render a certain portion of heat latent.

Let us now seek the rational explanation of these facts.

We have the formula $mc t$, which gives the quantity of heat contained in any body.

Let m be the mass expressed in kilogrammes,
 c the specific heat of the body,
 t its temperature.

But here the factor m must be abstracted, because there is no contact between the hand and the metal in fusion, and the

experiment presents no difference, being made either with 10 kilogrammes of metal, or with 1000 kilogrammes. The sensation which is felt is the same in either case, and this is readily conceived, knowing the repulsive force of incandescent surfaces which is opposed to the contact of any body.

The finger or the hand is then isolated in the midst of the mass in fusion, and thus preserved from the disorganizing action of the incandescent matter. I repeat, that the mass must be abstracted.

There remain the two factors c, t . I will suppose, and it is a sufficient approximation, that the value of $c=0.15$, and that of $t=1500$ degrees, the temperature of the metal in fusion; now the product of 1500 degrees $\times 0.15=225$. Thus the epidermis of the experimenter would only be exposed to 225 degrees of heat. Undoubtedly this is a respectable quantity of caloric, but it is too high, as we shall see.

There is no contact between the hand and the metal; this, in my estimation, is a fact positively established. If there is no contact, the heating can only take place by radiation, and it is enormous, it must be acknowledged; but if the radiation is annulled by reflexion, and it is so, it is as if it did not exist, and, definitively, the operator is, so to say, placed in normal conditions.

I think that I have established, a long time ago, the fact that water in the spheroidal state has the property of reflecting radiating heat*, and that its temperature never attains that of its ebullition; whence it follows that the finger or the hand being humid, cannot rise to the temperature of 100° Centig., the experiment not continuing long enough to permit the humidity to evaporate entirely.

To recapitulate what I have stated on this point, I say,—in passing the hand into any metal in fusion, it becomes isolated; the humidity which covers it passes into the spheroidal state, reflects the radiating caloric, and does not become heated enough to boil. This is all.

I was right then in saying at the outset, this experiment, dangerous in appearance, is almost insignificant in reality.

I have often repeated it with lead, with bronze, &c., and always with the same success†.

* Nouvelle branche de Physique, or Études sur les Corps à l'État sphéroïdal, pp. 24 *et seq.* and 132 *et seq.* See also our two letters to the Académie des Sciences, dated the 14th and 21st of July, 1845. In the places indicated will be found the explanation of this phænomenon.

† The experiments on the cast iron were made in the foundry of M. Davidson, at La Villette; and, on the bronze, in that of M. Nérat, Rue Pierre-Levée. I am happy to have an opportunity of publicly thanking these gentlemen for their kind assistance.

IX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxxiv. p. 532.]

March 8, " **A**DDITIONAL Observations on the Osteology of the 1849. Iguanodon and Hylæosaurus." By Gideon Algonon Mantell, Esq., LL.D., F.R.S., V.P.G.S., &c.

This memoir is supplementary to the author's former communications to the Royal Society on the same subject, and comprises an account of some important additions which he has lately made to our previous knowledge of the osteological structure of the colossal reptiles of the Wealden of the South-east of England.

The acquisition of some gigantic and well-preserved vertebræ and bones of the extremities from the Isle of Wight, and of other instructive specimens from Sussex and Surrey, induced the author to resume his examination of the detached parts of the skeletons of the Wealden reptiles in the British Museum, and in several private collections; and he states as the most important result of his investigations, the determination of the structure of the vertebral column, pectoral arch, and anterior extremities of the Iguanodon. In the laborious and difficult task of examining and comparing the numerous detached, and for the most part mutilated bones of the spinal column, Dr. Mantell expresses his deep obligation to Dr. G. A. Melville, whose elaborate and accurate anatomical description of the vertebræ is appended to the memoir. The most interesting fossil remains are described in detail in the following order.

Lower Jaw.—Since the author's communication on the lower jaw of the Iguanodon, published in the Philosophical Transactions, part ii. 1848, he has discovered the right *angular* bone, which was previously unknown: from the circumstances under which this relic was found, he considers it probable that it belonged to the same individual as the teeth figured in Plate XVIII. of the Philosophical Transactions for 1848.

Vertebral column.—The vertebræ hitherto assigned to the Iguanodon consist of the middle and posterior dorsal and anterior caudal, as identified by means of the Maidstone specimen in the British Museum: the cervical, anterior dorsal, lumbar, and posterior and terminal caudals, were previously either undetermined or referred to other genera of saurians. The investigations of Dr. Melville have established the important and highly interesting fact, that the cervical and anterior dorsal vertebræ of the Iguanodon were convexo-concave—that is, convex in front and concave behind—as in the fossil reptile of Honfleur termed *Streptospondylus*, and in the existing pachyderms; the convexity gradually diminishing, and the anterior face of the body of the vertebra becoming flat, in the middle and posterior part of the dorsal region. The supposed Streptospondylidæ vertebræ of the Wealden (named *S. major* by Professor Owen in British Association Reports on fossil reptiles) are, in the opinion of the author and Dr. Melville, the true cervical vertebræ of the

Iguanodon. The convexo-concave type of vertebræ was not confined to a single genus—the *Streptospondylus* of the Oolite—but prevailed in two, and probably in several, genera of extinct saurians of the secondary geological epochs; in like manner as the reverse form, the concavo-convex, predominates in the existing crocodilians and lizards.

Other large vertebræ found with ribs and bones of the extremities of the Iguanodon, and referred by Professor Owen to one or more species of *Cetiosaurus*, are regarded, in consequence of the peculiar structure of the neural arch, as belonging to the posterior dorsal and lumbar vertebræ of the former colossal reptile; and certain somewhat angular vertebræ, also previously assigned to a species of *Cetiosaurus*, are presumed to be the middle and distal caudals of the Iguanodon.

The Sacrum, of which portions of several examples belonging to individuals of much disparity in size have been obtained, is shown to consist of *six* anchylosed vertebræ; not of *five*, as stated by Professor Owen; and the typical specimen in the possession of Mr. Saull, which the author figures and describes, is adduced in proof of the correctness of this opinion. The anterior vertebra, and the two posterior ones, are much larger and stronger than the three intermediate elements which occupy the centre of the arch of the sacrum.

Pectoral arch.—A perfect scapula discovered in the strata of Tilgate Forest, and which corresponds with the coracoid bone, provisionally assigned to the Iguanodon in the memoir of 1841 (*Phil. Trans.* Pl. IX. fig. 11), Dr. Mantell has been enabled to refer to that reptile, by the fortunate interpretation of portions of two scapulæ which are preserved in the Maidstone specimen, but had not previously been recognized as such. As the clavicles were long since determined, the essential elements of the pectoral arch are now ascertained, and the author gives a restored outline of this important part of the skeleton, based upon these data.

Humerus.—A humerus three feet long, discovered by Mr. Fowles-tone in the Isle of Wight, has been ascertained by the author to belong to the Iguanodon, from the presence of a small but corresponding bone in the Maidstone fossil. This bone, from its disproportionate size in comparison with the femur with which it is collocated—being one-third shorter—was formerly assigned by Dr. Mantell to the fore-arm; but the large humerus from the Isle of Wight, which, except in magnitude, is identical with that from Maidstone, leaves no doubt upon the subject. It is now therefore, for the first time, ascertained, that in the Iguanodon, as in many fossil and recent reptiles, the anterior extremities were much shorter and less bulky than the posterior. The radius and ulna are still undetermined, but the author states that there are some imperfect bones in his former collection, now in the British Museum, which he thinks will be found to belong to the fore-arm.

Hinder extremities.—The colossal magnitude of the Iguanodon is strikingly shown by some femora and leg-bones recently discovered. One *femur* is 27 inches in circumference, and must have been 4 feet

8 or 10 inches in length; and a tibia, found with the same, is 4 feet long.

Dermal scutes and spines.—The author figures and describes several dermal scutes and spines, and states that a microscopical examination of the large angular bones of the *Hylæosaurus* (Phil. Trans. 1841, Pl. X. fig. 1), supposed by him to be ossified dermal spines, but which Professor Owen regarded as the abdominal extremities of ribs, proves the correctness of his own opinion; their structure being identical with that of the acknowledged dermal scutes.

In the summary which concludes the memoir, Dr. Mantell states that the facts described, confirm in every important point the physiological inferences relating to the structure and habits of the *Iguanodon* and *Hylæosaurus*, enounced in his former communications; and thus, after the lapse of a quarter of a century, he concludes his attempts to restore the skeletons of the colossal saurian herbivores, of whose former existence a few water-worn teeth and fragments of bones were the only indications, when, in 1825, he first had the honour to submit to the Royal Society a notice on the teeth of the *Iguanodon*.

March 15.—“Researches in Physical Geology.” Part II. By Henry Hennessy, Esq. Communicated by Major Beamish, F.R.S.

In this communication the author states that, having in Part I. (read to the Society in December 1846) endeavoured, by generalizing the hypothesis on which is usually founded the theory of the earth's figure, not only to improve that theory, but also to establish a secure basis for researches into the changes which may have taken place, within and at the surface of the earth, during the epochs of its geological history, his object here is to discover relations between the interior structure of the earth and phenomena observed at its surface, and also the effects of the reaction of the fluid nucleus, described in Part I., upon the solid crust. This memoir is divided into sections, each containing a distinct investigation; and the statement of the geological results is given at the end.

I. The Pressures of the Shell and Nucleus at their surface of contact.

In the investigation of these pressures the earth is supposed to consist of a nucleus of fluid matter inclosed in a solid shell, the inner and outer surfaces of which are spheroidal, but nearly spherical; and both shell and nucleus are supposed to consist of strata varying in density according to some unknown inverse law of the radii. The pressure at the inner surface of the shell is conceived to be due to a constant pressure, which is the same for every point, and a variable pressure, arising from the difference in form of the surface of the nucleus and inner surface of the shell. On these suppositions, simple expressions for the pressure on any stratum of the nucleus and on the shell's inner surface are deduced.

II. The Variation of Gravity at the earth's surface.

The author does not assume in this investigation that the laws of arrangement of the particles composing the shell and the nucleus

are necessarily the same; so that the expression which he obtains for gravity at any point on the earth's surface, besides being a function of the latitude of that point, and of the radii and ellipticities of the shell's inner and outer surfaces, contains functions depending on the constitution of the shell and nucleus. He states that this expression for gravity is not merely speculative, but that it will be found to assist in explaining certain apparent anomalies detected by observation in the variation of gravity at the earth's surface, as well as in pointing out the limits assigned by observation to the thickness of the crust.

III. *The Laws of Density of the Shell and Nucleus.*

According to the author's views in a subsequent section, it appears that the solidification of the earth could not proceed simultaneously from the centre towards the surface, and from the surface towards the centre. He therefore, in determining the laws of density of the shell and the nucleus, restricts his investigations to the latter case, in which the solidification proceeds from the surface towards the centre.

IV. *The Forms of the Strata of the Shell.*

The author conceives a surface to exist which may be called the effective surface of separation of the perfect fluid of the nucleus and the imperfectly fluid portion adhering to the shell, the form of which surface will depend on the pressures which the fluid exerts. As it may be shown that the pressure of the perfect fluid will not be constant, the surface of separation will tend to assume a form different from that of the inner surface of the shell. If we admit that the matter composing the nucleus becomes denser in assuming the solid state, the author concludes that the inner surface of each stratum added to the shell will be more oblate than its outer surface; and that thus the tendency will always be to render the inner surface of the shell more and more oblate. He then deduces an expression for the ellipticity of the fluid surface.

V. *The principal Moments of Inertia of the Earth.*

From his investigations the author concludes that, as the thickness of the shell increases, the difference between the greatest and the least moment of inertia of the earth also increases; which conclusion is independent of any knowledge of the absolute laws of density of the earth's interior.

VI. *On the existence of a Solid Nucleus within the Earth.*

The conclusion arrived at here is, that if a solid nucleus existed, as the pressure on it would be continually diminishing, while its temperature would remain nearly constant, this nucleus, instead of increasing in magnitude, would tend to return to its original fluid state.

VII. *The directions of the Fissures in the Shell which might be produced by the action of the pressures in Section I.*

The author states that the tendency of the variable pressure is in the first instance to produce fissures parallel to the equator; that when such a fissure was once commenced the tendency would be to propagate it along a parallel of latitude, until the force of the tensions became sufficiently lessened by the separation of the extended portion of the shell; and that similar fissures would be formed simultaneously and symmetrically on each side of the equator. Subsequently, as may readily be deduced from Mr. Hopkins's investigations, the tendency will be to form fissures at right angles to those previously existing. If, however, the constant pressure were far greater than the variable, the directions of the fissures would be governed chiefly by accidental causes; but if a fissure commenced, it would continue to be propagated in the great circle coinciding with its first direction, unless accidental causes should alter its course.

VIII. *On the existence of a Zone of least disturbance in the Shell.*

The author investigates analytically the position of this zone, and from the results of his investigation, points out the conditions under which it will exist, and also the consequences that will follow from its non-existence.

IX. This section is devoted to the calculation of some of the constants contained in the formulæ of the preceding sections.

The following are the geological deductions from the foregoing investigations:—

1. The stability of the axis of rotation of the earth will progressively increase during the process of solidification.

2. By employing the values of the constants obtained in Section IX., it appears that the thickness of the earth's crust cannot be less than 18 miles, and cannot exceed 600 miles.

3. The earth's primitive ellipticity, when entirely fluid, was less than its present ellipticity; but their difference may be neglected.

4. If a zone of least disturbance existed near the parallel of mean pressure, the directions of great lines of elevation should be in general parallel or perpendicular to the equator. Its non-existence there, which observation seems to show, proves at least that the variable pressure did not predominate over the constant. Since, as yet, observation goes to prove that such a zone does not exist on the earth's surface, we must provisionally conclude that the constant pressure greatly predominated over the variable, and, consequently, that the directions of the lines of elevation must be comparatively arbitrary.

5. That great friction and pressure exist at the surface of contact of the nucleus and shell, is shown from the conclusions arrived at in Section IV., combined with the important result obtained by Mr. Hopkins in his second memoir on Physical Geology (Phil. Trans. 1840, p. 207).

6. The amount of elastic gases given off from the surface of the nucleus rapidly decreases as the thickness of the shell increases.

7. The expression obtained for the variation of gravity shows that, if the angular velocity of rotation of the earth remained unchanged, the waters on its surface would tend to accumulate towards the equator, for the increase of gravity, in going from the equator to the poles, would be less according as the shell's thickness increased.

March 22. —“An Account of the Aurora Borealis of the 17th of November 1848.” By the Rev. Charles F. Watkins. Communicated by the Marquis of Northampton, V.P.R.S.

The author states that: “About half-past 7 P.M. the sky assumed the appearance which it usually does immediately preceding the action of what are called the Northern Lights. In the northern half it was quite clear for about forty-five degrees from the meridian, of a pale blue, and covered with a faint light, such as generally ushers in the moon at her rising. Towards the east and west this light gradually diminished, and south of those cardinal points the dimness as gradually thickened.

“Soon after eight the coruscations began by the usual lambent strokes of a shining filmy matter, like the sudden shooting forth and instantaneous retroceding of a serpent's tongue. They commenced in the north-east, and shot upwards in an angle of about 70 degrees of inclination towards the south, and to about 60 degrees in length, more or less, leaving the sky clear to the north, and in a manner gradually chasing the clouds, upon whose receding bounds they glanced further to the south.

“In a short time the same kind of electrical action commenced in the north-west quarter of the heavens, and continued simultaneously with that from the north-east, both increasing in rapidity, intensity and depth of colour; till at length an entire hemispherical arch of crimson and purple, but with uneven edges, spanned the heavens from east to west, and remained suspended there for several minutes. By degrees this arch broke up into separate masses of highly and parti-coloured clouds, resembling those which are seen floating about after the setting of an ardent sun. Meanwhile the lighter coruscations continued,—now glancing upwards on the northern edges of the clouds, which were still slowly receding to the south, and now shooting up beneath them as they steadily retreated. At the same time others of a redder hue played now alternately, and now in union with them.

“About a quarter past nine an extraordinary phenomenon occurred, such as I never before witnessed; the zenith assumed the appearance of a crimson coronary apex to distinct but connected bands of various shades of crimson, green and purple, in which the crimson prevailed, flowing down from thence like a canopy, encircling the upper portion of the heavens, which to me presented the inside view of a ribbed and vaulted cupola. By degrees this beautiful creation dissolved, and the body of clouds, against which the electrical forces seemed to have been in hostile pursuit, fled away to the south; the elementary action ceased: a silent calm returned, and nothing but the tranquil light, still shining in the north, remained to indicate the recent scene. The wind had blown with a

fresh but steady breeze from the north-west, during the continuance of the phenomenon.

"Without entering at present into any disquisition upon the causes, I will now state the meteorological results which I immediately anticipated and have seen to follow these atmospheric phenomena.

"I have observed, and have stated my observations for some years past, that the certain result of all meteoric coruscations and iridescences in the sky, is a fall of rain, snow or hail,—on this general principle, that the condensation of the crystalline particles of floating vapours which ensue upon electrical action, must be followed by precipitation; and these coruscations and iridescences are both the reflected evidences of such condensation of crystalline matter, and therefore the harbingers of such precipitation. It is the case with solar and lunar rainbows, falling stars, mock-suns, halos, lightning, aurora, and that undefined pearly lustre which sometimes appears in the neighbourhood of the sun.

"Accordingly, on the following morning, Saturday the 18th, I found the barometer had sunk considerably, and the wind had veered round from north-west to south-west, against the course of the sun, both in general, and especially when united, the forerunners of rain. Accordingly at 2 o'clock P.M. a smart shower came on in Northampton, but was of short duration. At 9 P.M. a heavier shower was experienced at Brixworth; and in the course of the night, but I cannot say at what hour, I was awakened to a still heavier shower; but the quantity of rain that had fallen did not seem to have affected the ground much on the following morning, and therefore I conclude that it was not great.

"Sunday the 19th was fine and bright; the wind went up to the westward, and the barometer rose rapidly—a general indication of an early change. Towards morning of Monday the 20th, another shower fell, and the wind went back to the south-west with a falling barometer. In such cases I generally find that rain ensues about midday, or at least when the wind and sun meet in the south-west. But on this occasion it continued blowing strong all the day, and for some time in the night with increased violence. But at last the wind fell, and was succeeded for awhile by heavy rain, thus verifying my anticipations on this particular occasion, and the general theory which I have discussed."

April 19.—"On the Meteorology of the Lake District of Cumberland and Westmoreland." By John Fletcher Miller, Esq. Communicated by Lt.-Colonel Sabine, R.A., For. Sec. R.S., &c.

This paper contains the results of meteorological observations made during 1848, similar to those made in the same district in preceding years, which were last year communicated to the Society. On these results the author remarks that the fall of rain in the lake district, during the year 1848, greatly exceeds the amount in any other year since the register was commenced in 1844; and that there is a similar excess with reference to the number of wet days. The total depth of rain, in 1848, at Seathwaite, the wettest station, was 160·89 inches; and of this quantity, 114·32 inches fell in the

six months, February, July, August, October, November and December. In February there fell the unprecedented quantity 30·55 inches.

The mountains flanking the lake-district valleys increase in altitude with great regularity towards the head or eastern extremity of the vale, and it appears that it is there that the greatest depth of rain is invariably found. The amount increases rapidly as the stations recede from the sea, and towards the head of the valley the incremental ratio is exceedingly great. At Loweswater, Buttermere and Gatesgarth, about two miles apart in the same line of valley, the depths of rain were respectively 76 inches, 98 inches and 133·5 inches.

From the observations of the thermometer, the author concludes that the climate in the mountain valleys in this district is milder and more equable, not only than in the open country in their immediate vicinity, but also than in that considerably to the south. This he attributes to the lakes giving out during the winter the heat absorbed by them in the summer, and to the radiation from the rocky mountain breasts in the valleys, but principally to the heat evolved in a sensible form by the condensation of enormous volumes of vapour.

Last summer a pair of Rutherford's self-registering thermometers were stationed by the author on the summit of Sca Fell Pike. He states that from the maximum thermometer no correct readings could be obtained; but that the minimum gave the following:—July, 22°: August, 24°; September, 18°; October, —6°; November, —6°; December, —9°. It appears that on the night between the 2nd and 3rd of January the minimum thermometer indicated the extraordinary low temperature —34° Fahr.: at the same date a naked thermometer on grass at Whitehaven fell to +4°, and one on raw wool to —2°·8.

The author states that the results obtained from the mountain gauges during the last year, are in strict accordance with those of the two preceding years, and thus confirm the correctness of the conclusion drawn from them in his former paper, "that the quantity of rain increases from the valley upwards to an altitude of about 2000 feet, above which it begins to diminish." He does not, however, by any means infer that the law which appears to regulate the distribution of rain in the mountain district of Cumberland will equally apply to every similar locality.

X. Intelligence and Miscellaneous Articles.

ON THE AURORA BOREALIS OF FEBRUARY 22, 1849.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I BEG leave to occupy a small space in your valuable journal for a notice of the Aurora Borealis which I observed on the 22nd of February of this year.

About ten minutes to seven there was a low arch of white light,

with many streamers; the height of this arch, estimated by β Draconis, was about 8° . Again, at $7^h 45^m$, there was a brilliant display of streamers, and an arch which was about 12° in altitude. There was also at the height of 3° or 4° a long sinuous line of white light. A day or two afterwards I was in Northamptonshire, not far from Banbury, and found that on the night in question the arches had been noticed at considerable altitudes: my observation then became of use; for having a base line of as nearly as possible twenty miles, and *the small angle accurately taken*, the angle at the other end *being large*, a considerable inaccuracy in *this angle* will not greatly affect the result. If we take the crown of the arch to have been in the zenith of this station at $7^h 45^m$ P.M., as I was informed at that time it was about over head, and could not be seen without going out of doors, we shall find the height of the crown of the arch to have been $4\frac{1}{4}$ miles, or about 22,400 feet. From a rough estimate of the space which the arch subtended in azimuth, the distance between its feet was twelve or fourteen miles, which rested, as it appeared, on confused masses of auroral light in the N.E. and N.W., which must have been about the height of the ordinary cirrus. The true shape of the arch therefore appears to have been a long flat curve, of which the subtense was equal to six or seven times its altitude.

It seemed also that the long sinuous line which I mentioned as seen through the arch was formed of a number of these arches at a much greater distance, the direction of which was not in a straight line, though possibly parallel to one another.

From frequent observation, I believe that the streamers which usually attend the display of Aurora are parallel to the earth's surface and to one another, and that their convergence is apparent only, and produced by perspective. Probably also, whenever the electricity in this state is in sufficient quantity and of opposite kinds, arches are formed, the true shape of which appears to resemble the magnetic curve. The *varying* proximity of these electro-magnetic clouds will also very well account for the disturbance of the needle.

I am, Gentlemen,

Your obedient Servant,

Rose Hill, Oxford, June 15.

JOHN SLATTER.

ON LIQUID STORAX AND BALSAM OF PERU. BY M. KOPP.

The author states, that having in preceding memoirs studied the resins of benzoin and the balsam of Tolu, there remained to complete the general history of the balsams to examine the balsam of Peru and storax.

Balsam of Peru had already yielded MM. Frémy and Plantamour very interesting results. M. Frémy had determined the presence of cinnamic acid, cinnamein and several resins: in some species of balsam of Peru he met with a solid crystalline matter, to which he gave the name of metacinnamein. According to this chemist, when cinnamein is submitted to the action of potash dissolved in alcohol, it is converted into cinnamic acid, and peruvén, which is a liquid body.

According to M. Plantamour, there are besides formed under these circumstances cinnamic æther and a new acid, the carbo-benzoic.

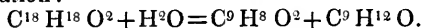
M. Kopp states that his researches confirm the results of M. Frémy, but not those of M. Plantamour.

Storax has been examined with much care by M. Simon of Berlin, who found an essential oil in it, styrol, a crystalline body, styracine, cinnamic acid and several resins. He stated that styracine is transformed by potash into cinnamic acid and liquid styracone.

These results are quite exact, only the author has not assigned the true composition to the isolated bodies.

The author observes that he has already shown that styrol is identical with cinnamen or cinnamol ($C^{16}H^{18}$), which is obtained by the dry distillation of cinnamate of copper, or of a mixture of cinnamic acid and caustic barytes. M. Kopp's researches also prove that great analogy of composition exists between storax and balsam of Peru.

Styracine, which may exist either in the amorphous or viscous state, or crystallized, is identical in the former condition with cinnamen, and in the second with metacinnamen. Its formula is $C^{18}H^{18}O^2$. Peruven is identical with styracone, its formula being $C^9H^{12}O$. The transformation of styracine into cinnamic acid and styracone under the influence of caustic potash, is expressed by the following equation:—



Styracine. Cinnamic acid. Styracone.

Styracine then possesses strong resemblance to neutral fatty matters, which, under the influence of alkalis, are converted with the absorption of an atom of water into fatty acids and glycerine.

As storax is the most readily treated and least expensive of these different bodies, the author selected it for his experiments.

The first operation is to distil the storax with five or six times its weight of water, in a copper alembic furnished with a refrigeratory. An oil floats on the condensed water, which is to be poured off, dried by chloride of calcium, and distilled at $290^{\circ}F.$; it is then cinnamen or pure cinnamol. It is not advantageous to employ in the distillation water containing carbonate of soda in solution, for it causes the mass to rise too readily and afterwards to run over. The residue of the distillation is boiled with a weak solution of carbonate of soda, which removes the free cinnamic acid, and cinnamate of soda is formed which remains in solution. This operation is to be repeated many times, till at last, especially when the mixture cools, it becomes more and more spongy, and retains in its interstices a viscid, yellowish oily matter. The watery liquids containing the cinnamate of soda and a slight excess of carbonate, and also a little resinate of soda, are to be much concentrated, and then decomposed with excess of boiling hydrochloric acid. The greater portion of the impure cinnamic acid is deposited in the liquor in a concentrated state in the form of a heavy brown oil, which solidifies on cooling, and the aqueous liquid is filled with crystals of cinnamic acid. The resinous matter is to be removed, and the crystals, after being strongly pressed and washed with a little cold water, are to be distilled from a glass

retort. The first product of the distillation is cinnamic acid nearly pure; the last products are rendered impure by empyreumatic oils; by dissolving, however, this impure acid in boiling distilled water and filtering, the oil remains on the filter, and the filtered liquor is filled with very white and perfectly pure crystals of cinnamic acid. This is the most ready and least expensive process for obtaining this acid in a state of absolute purity.

The resinous, ductile, spongy matter containing the yellow opaque liquid in its pores, becomes when squeezed more and more compact, and the yellow liquid flows from it; this is to be filtered, the operation goes on very slowly, and the surface of the liquid readily solidifies. By this means a brownish yellow oily matter is obtained, which after some time becomes a stellar crystalline mass of impure styracine. In order to purify it, it is dissolved in about ten times its weight of alcohol at 122° F.; the solution is poured off and subjected to a low temperature. The styracine then crystallizes in very fine white flexible needles. The compact resin from which the styracine has been pressed becomes hard and brittle by cooling. It still retains a considerable quantity of styracine, and it is advantageous to employ it in the preparation of styracone; there is thus obtained an additional quantity of alkaline cinnamate, mixed with the resins; to effect this it is put into an alembic, with a concentrated solution of potash or soda, and distillation is to be cautiously performed. A milky liquid passes over, which when saturated with common salt yields on its surface a creamy matter, which gradually unites into an oily stratum; it is to be removed, filtered, and rectified. There is thus obtained a colourless liquid, which boils at about 490° F., crystallizes at a low temperature, becomes solid at 46°, and has a peculiar odour. It is styracone. In the alembic there remains a blackish brown and very alkaline liquid, in which there float numerous mammillated grains of a yellowish colour, consisting of a mixture of cinnamate and resinate of soda. The liquor may be filtered in a funnel containing broken glass; the resinate is to be washed with a little water, which, added to the alkaline liquor, may be employed to decompose a fresh quantity of storax. When water is added to the globules, a resinous matter is deposited, and the solution contains much cinnamate and some resinate of soda; the cinnamic acid and the resin are to be precipitated by hydrochloric acid; these two substances are to be separated by boiling water or solution of ammonia, which does not dissolve the resins. To obtain the latter in a pure state they must be successively washed with acidulated and ammoniacal water, and lastly with boiling water. The different resins are separated by means of their different solubility in alcohol, æther, and pyroxylic spirit. They are hard and brittle, not readily fusible, and but slightly coloured.

Styracine is the most interesting substance contained in storax. It may exist in two conditions: crystalline and fusible at 100° F., and liquid, viscid and uncrystallizable. When pure crystals of styracine have been melted, the liquid frequently does not solidify on cooling.

Sometimes also on operating on storax, the styracine is obtained in a liquid uncrystallizable state, especially when it has been left for

too long a time in contact with acids, for the purpose of separating the last traces of soda.

When styracine is treated with the caustic alkalies, it combines with them, and forms a solid mass consisting of agglomerated grains. By the application of heat, styracone distils, and cinnamic acid is obtained. Submitted to the action of chlorine, chlorinated styracine is obtained. By distilling this compound under the influence of chlorine, a volatile chlorinated liquid and a chlorinated crystallizable acid are procured; the latter very readily forms salts. With nitric acid, styracone forms oil of bitter almonds, hydrocyanic and nitrobenzoic acids; with chromic acid it gives hydruret of benzoil, benzoic acid and resin; with concentrated sulphuric acid, it yields cinnamic acid and a brown substance, soluble in water and insoluble in aqueous saline solutions.—*L'Institut*, Juin 6, 1849.

IDENTITY OF BROOKITE AND ARKANSITE.

We learn by a note from Professor Miller, dated the 2nd of May, that he has found an exact agreement between the forms and angles of a mineral from the United States, lately described by Professor Shepard, and named Arkansite, and the previously established species Brookite; and as both consist almost entirely of oxide of titanium, they must, we conclude, be regarded as varieties only of the same mineral.—Ed.

ON THE ESTIMATION OF MOLYBDIC ACID. BY M. H. ROSE.

Molybdic acid, MoO_3 , may be estimated in the state of sulphuret by precipitating its solutions, rendered acid, with sulphuretted hydrogen. This method possesses some inconveniences: on one hand, the precipitation always takes place slowly, and the washings, which have usually a blue colour, require to be heated with solution of sulphuretted hydrogen to precipitate the last portions of molybden: on the other hand, it is requisite to calcine the brown sulphuret of molybden obtained, in order to convert it into gray sulphuret MoS_2 , the weight of which serves for calculating the quantity of molybden.

M. H. Rose finds it more correct and convenient to estimate the molybdic acid by reducing it in a current of hydrogen gas to the state of perfectly fixed oxide of molybden. He uses in this operation a platina crucible, which has a tubulated cover, by which the hydrogen gas is conveyed. The heat of a spirit-lamp is to be employed, in order that the temperature may not be raised high enough to reduce any of the oxide of molybden to the metallic state. The weight of the oxide of molybden obtained serves for calculating that of the molybden or the molybdic acid.—*Journ. de Ph. et de Ch.*, Janvier 1849.

ON GLAIRINE. BY M. BONJEAN.

This substance is a vegeto-animal matter produced at the sulphurous spring of Aix in Savoy. According to M. Duby, who has examined it microscopically, it consists of extremely minute fragments of a plant, of an extraordinarily fine, close, undulating tissue,

which is insoluble in water, and has the appearance of an animal remain.

Glairine is produced by the immediate action of the air on the sulphurous water, and is deposited on the pavement of the pumps. It retains a large quantity of water, which it does not lose by long exposure to the air: it is not entirely expelled below 104° F. Thus dried it is quite colourless, completely inodorous, of a horny appearance, and is reduced to about one-tenth of its weight; when water is added to it, it is rendered again mucilaginous, becomes nearly of its original size, but remains inodorous. When dried and thrown upon burning charcoal, it gives the smell of burnt horn, without any trace of sulphurous acid, and the gases which it yields turn reddened litmus paper blue. The absence, in this experiment, of sulphurous acid seems to indicate that this substance contains no sulphur; it will soon be shown that it contains so little, that it can only be isolated by means of æther. Water, alcohol, oil of turpentine, nitric, hydrochloric, sulphuric, phosphoric, oxalic, acetic acids, &c., solution of chlorine, dilute alkalies, all dissolve a small quantity cold, and a little more when heated, but some of them occasion peculiar modifications in it. The nitric, concentrated hydrochloric acid, and liquid chlorine, immediately destroy the gray colour which it acquires out of water and restore its natural whiteness. Separated from these acids, it becomes more gray by exposure to the air: other acids do not decolorize it. Alkalies precipitate it from solution in acids in white flocculi which have a bluish reflexion. The nitric solution, filtered and evaporated to dryness in a small porcelain capsule, leaves a yellowish sharp residue, which is slightly acid, difficultly decomposable in a strong heat, and insoluble in water and in alcohol: glairine does not decompose nitric acid unless it be heated. When hydrochloric acid decolorizes glairine blackened by contact with the air, it assumes a yellow colour, derived from the formation of a persalt of iron. This shows that this organic matter contains peroxide of iron in combination, derived, unquestionably, from the carbonate of iron which the water holds in solution. Concentrated sulphuric acid, instead of decolorizing it, like the fore-mentioned acids, imparts to it the colour of wine dregs, which becomes lighter when acidulated water is added. The caustic alkalies do not act upon it when cold; but when heated a green colour results, which acids cause to disappear immediately. Bromine also decolorizes it; but at first it gives it a red colour, and the decoloration of the glairine is not perfect until all the bromine is volatilized. If it should retain a yellow tint from some remaining bromine, washing with distilled water renders it perfectly white. Iodine colours it brick-red; and this colour does not disappear, either by long exposure to the air or frequent washings. Alcohol and oil of turpentine dissolve a small quantity, and acquire a slight yellow tint; alcohol becomes sweetish, and its consistency is sensibly increased. If these two liquids be evaporated to dryness in a porcelain capsule, the lower portion is carbonized by slightly increasing the heat.

Glairine is totally insoluble in æther. If they be mixed in a well-stopped phial, and shaken occasionally during two or three days, the

æther when filtered leaves by evaporation small rounded grains of perfectly pure sulphur; they are yellow and brilliant, of the size of a pin's head, and when thrown on a burning coal burn with a fine blue flame and the disengagement of sulphurous acid.

When glairine is gradually heated on a platina crucible, it loses its interposed water slowly, and even begins to be decomposed before it has parted with the whole of it; towards the end of the calcination it exhales the odour of burnt horn, without any sensible disengagement of sulphurous acid, and leaves a coaly residue. It does not fuse, and it is difficult to incinerate it; it does not lose its odour by numerous washings with cold water, but imparts a very distinct one to it, with a sweetish taste, without giving it colour; long boiling takes away the greater part of it; the solution has a strong smell, and the residue is very small. This liquor when filtered is yellowish, has a sweetish animal taste, is not mucilaginous, and does not coagulate on cooling; when evaporated to a syrupy consistence in a porcelain capsule, it colours the sides of it strongly yellow. The residue is of a deep yellow, with a slight smell and bitterness; the sulphuric and nitric acids, whether concentrated or diluted, do not sensibly act upon it. When glairine is subjected to dry distillation in a glass retort placed in a reverberatory furnace, it swells at first, and boils in the water which it retains interposed. This water soon begins to distil by drops, is colourless, has a strong smell of animal matter, and the apparatus is filled with vapours. The odour of burnt horn soon replaces that of animal matter, and drops of a yellow colour then fall into the receiver. Lastly, when the heat becomes very strong, the retort contains charcoal, and the neck is covered with a black substance, of which a few drops only fall into the receiver with the yellow liquor and immediately solidify. The yellow distilled liquor has a strong burnt odour and taste, with slight bitterness. It reddens litmus feebly, and dissolves in all proportions in water and alcohol. Æther does not dissolve it, but separates a small quantity of yellow fatty matter, which it leaves by evaporation; it is insoluble in water, but very soluble in alcohol.

The coaly residue of the distillation treated with distilled water yielded a slightly alkaline solution; during distillation the gases disengaged restored the blue colour of reddened litmus. The dry residue of the distillation was a very light, black and friable charcoal, which yielded 0.75 of ash composed of silica, carbonate of lime, and peroxide of iron. No trace of iodine could be detected.

The preceding experiments on glairine lead to the following conclusions: it contains very little nitrogen and no iodine; it dissolves sparingly in water, alcohol, oil of turpentine, and rather more readily in concentrated acids, from which the alkalies precipitate it in bluish-white flocculi; heat in all cases increases the solvent power of the liquids; it is quite insoluble in æther, which isolates perfectly the small quantity of sulphur which it retains interposed between its molecules; it becomes rapidly of a more or less blackish-gray colour when taken from the water and exposed to the air; but it is sufficient to treat it with nitric or hydrochloric acid, bromine or

chlorine, to restore its natural whiteness: sulphuric acid, far from decolorizing it, imparts to it the colour of wine-lees; the concentrated alkalies render it green when heated, and the alkalies destroy it; when in water it has but very little odour, but as soon as taken from it, it acquires a most disgusting smell, which is not dissipated by long exposure to the air, at least while it retains a little water; nor is it got rid of by much washing with cold water, or by long boiling, although in the latter case the greater part of it disappears. Lastly, it becomes perfectly inodorous by thorough drying in a stove, assumes a horny appearance, and is reduced to about one-tenth of its weight.—*Journ. de Ph. et de Ch.*, Mai 1849.

ON GLAIRIDINE. BY M. BONJEAN.

The author observed that when the sulphurous waters above described become mixed with rain-water, another vegeto-animal matter appears, to which he has given the name of *glairidine*.

The principal characters of this substance are, that it is of a deep gray colour, instead of being colourless, like glairine; it is inodorous, and remains so even when exposed to the air. Long exposure to the air does not alter its colour; but if a glass bottle be immediately filled with it, it soon acquires a smell, which in a few days becomes as disagreeable as that of glairine taken from water. If it then be taken from the bottle and exposed to the air, it becomes quite inodorous, and dries perfectly in a few days; on the contrary, it has been shown that glairine does not lose its interposed water till exposed to a heated stove. Glairidine is not decolorized either by any acid or by liquid chlorine. Like glairine, it renders hydrochloric acid yellow on account of the peroxide of iron which it contains. Water, alcohol, oil of turpentine, and the acids, dissolve a small quantity of it; it is insoluble in æther; it separates sulphur, but in so minute traces, that to perceive them it is requisite to operate on a great quantity of the matter. The caustic alkalies do not render it green, either cold or hot. If it be thrown on a filter, it retains a little water; and when afterwards dried on a stove, it loses only two-thirds of its weight. In this state, instead of having a horny appearance, like glairine, it presents a uniform, friable, solid mass, and does not swell in water. The water which runs through the filter is as inodorous as the substance itself, and it contains a very small quantity of *zoiodine*. When decomposed in a glass tube, it exhales the odour of burnt horn, and yields gases which strongly restore the blue colour of reddened litmus. Lastly, glairidine yielded by analysis very evident traces of iodine, which, as already stated, glairine did not.—*Ibid.*

ON ZOIODINE. BY M. BONJEAN.

The author has given this name to a new substance from two Greek words, expressive of its azotized nature and its violet colour. In order to obtain it, it is requisite to employ very white glairine, which cannot be procured except when the sulphurous waters are in a state of perfect purity, and nearly at their maximum of sulphuration. This substance exists in the state of strongly iridescent scales of a fine deep violet colour; it has neither taste nor smell; it is un-

alterable by the action of light and air, and is insoluble in water. By concentrated nitric acid it becomes of a yellowish red colour, and of a rather deeper yellowish red by hydrochloric acid; concentrated sulphuric acid renders it of a fine blood-red colour. Oxalic and phosphoric acids also redden it, but not so strongly as the preceding acids; acetic and arsenic acids produce no sensible effect; and in all cases diluted acids act almost as powerfully as when concentrated. Chlorine does not alter it; alkalies render it of a brown colour, which the stronger acids bring back to red. Lastly, when heated in a glass tube, it decomposes without subliming, and leaves a coaly residue, upon which acids exert no action. During calcination it yields a smell of burnt horn, and evolves ammoniacal vapours which restore the blue colour to reddened litmus.—*Journ. de Ph. et de Ch.*, Mai 1849.

METEOROLOGICAL OBSERVATIONS FOR MAY 1849.

Chiswick.—May 1. Cloudy. 2. Foggy: overcast. 3. Foggy: fine. 4. Very fine. 5. Clear and fine: thunder, lightning, rain and hail in afternoon: cloudy at night. 6. Overcast. 7, 8. Overcast and cold: fine: cloudy. 9. Fine: showery. 10. Overcast: slight rain. 11. Cloudy and cold. 12. Fine: overcast. 13. Very fine. 14. Rain: fine. 15. Cloudy: fine. 16. Rain: cloudy. 17. Cloudy: slight rain. 18. Overcast. 19. Cloudy and fine. 20. Rain throughout. 21. Hazy. 22. Rain: fine. 23. Fine. 24. Very fine: densely overcast at night. 25. Cloudy: very fine. 26. Overcast: very fine. 27. Very fine: cloudy: rain. 28. Overcast: very heavy rain. 29, 30. Very fine. 31. Dry haze: overcast: clear at night.

Mean temperature of the month	55°·19
Mean temperature of May 1848	58 ·12
Mean temperature of May for the last twenty-three years ...	54 ·22
Average amount of rain in May	1·82 inch.

Boston.—May 1. Cloudy. 2. Cloudy: rain early A.M. and late P.M. 3—5. Fine. 6—9. Cloudy. 10. Rain: rain A.M. and P.M. 11, 12. Cloudy. 13, 14. Cloudy: rain P.M. 15. Cloudy. 16. Rain: rain A.M. and P.M. 17. Fine: rain A.M. and P.M. 18. Rain: rain A.M. and P.M. 19. Cloudy. 20. Rain: rain A.M. and P.M. 21. Cloudy: rain P.M. 22. Cloudy: rain, with thunder and lightning P.M. 23. Cloudy: rain P.M. 24. Fine. 25. Rain: rain, with thunder and lightning early A.M. 26. Fine. 27. Rain: rain early A.M.: rain P.M. 28. Cloudy: rain early A.M. 29. Fine: rain early A.M. 30, 31. Fine.

Applegarth Manse, Dumfries-shire.—May 1. Remarkably fine day. 2. Dull, but fair. 3. Fiery heat: dry and parching. 4. Fiery heat. 5, 6. Fiery heat: heat less. 7. Fiery heat: a few slight drops of rain. 8. Fiery heat. 9. Mild day: wind variable. 10. Mild day: shower on the hills. 11. Chill and piercing: ungenial. 12. Mild and genial: rain at night. 13. Dropping day: most welcome rain. 14. Wet morning: bright afternoon. 15. Mild and damp: showers. 16. Heavy showers. 17. Wet morning: very fine and hot. 18. Slight showers: fine cool evening. 19. Hot forenoon: blowing evening. 20. Heavy showers: dull. 21, 22. Very fine day: damp evening. 23. Showers in forenoon: very fine. 24. Fair, but dull. 25. Fair and clear: cloudy P.M. 26. Fair and very fine. 27. Beautiful day. 28. Beautiful day: still warmer. 29. Fine, though cloudy: showers P.M. 30. Fine: clear bracing weather. 31. Slight rain: wind high P.M.

Mean temperature of the month	50°·5
Mean temperature of May 1848	52 ·9
Mean temperature of May for twenty-five years	51 ·09
Rain in May for twenty years	1·69 inch.

Sandwick Manse, Orkney.—May 1. Fine: clear. 2. Cloudy. 3. Clear: fine. 4. Fine. 5. Cloudy: fine. 6. Cloudy: clear. 7. Cloudy: fine. 8. Cloudy. 9. Bright: cloudy. 10. Fine. 11, 12. Cloudy. 13. Rain: fine. 14. Fine: drizzle. 15, 16. Cloudy: drizzle. 17. Cloudy: rain. 18. Drizzle: cloudy. 19. Clear. 20, 21. Cloudy. 22. Cloudy: hazy. 23. Bright: clear. 24. Bright: rain. 25, 26. Bright: clear. 27. Cloudy: clear. 28. Bright: cloudy. 29. Cloudy. 30. Bright: clear. 31. Bright: cloudy.

Days of Month.	Barometer.				Thermometer.				Wind.				Rain.					
	Chiswick.		Orkney Sandwick.		Chiswick.		Dumfries-shire.		Orkney Sandwick.		Chiswick.		Dumfries-shire.		Orkney Sandwick.			
	Max.	Min.	8 $\frac{1}{2}$ a.m.	9 a.m.	9 a.m.	8 $\frac{1}{2}$ p.m.	Max.	Min.	9 $\frac{1}{2}$ a.m.	8 $\frac{1}{2}$ p.m.	Max.	Min.	9 $\frac{1}{2}$ a.m.	8 $\frac{1}{2}$ p.m.	Chiswick.	Dumfries-shire.	Orkney Sandwick.	
1849. May.																		
1.	30.141	30.008	29.72	30.17	30.05	29.98	29.92	56	39	49	65 $\frac{1}{2}$	39 $\frac{1}{2}$	51	47	ne.	n.	se.	ese.
2.	29.956	29.854	29.58	30.00	29.94	29.86	29.85	65	48	49	57	43	52	46	ne.	nne.	e.	ese.
3.	29.810	29.797	29.45	29.94	29.97	29.87	29.87	74	44	58	67	46	55	46	e.	ne.	ne.	se.
4.	29.840	29.822	29.46	29.93	29.88	29.86	29.85	77	47	61	67 $\frac{1}{2}$	41 $\frac{1}{2}$	50	45	ne.	ne.	e.	e.
5.	29.819	29.781	29.43	29.92	29.90	29.82	29.84	74	45	58	55	43	47	43	ne.	nne.	ne.	ne.
6.	29.843	29.813	29.50	29.91	29.99	29.85	29.88	62	45	49	53	40 $\frac{1}{2}$	47	43 $\frac{1}{2}$	ne.	ne.	nne.	nne.
7.	29.989	29.889	29.62	30.03	30.09	29.88	29.97	55	36	46 $\frac{1}{2}$	54	37	48	43	ne.	ne.	ne.	ne.
8.	30.015	29.994	29.70	30.11	30.10	29.97	29.90	55	39	48	55	36 $\frac{1}{2}$	47	43	ne.	ne.	e.	0.00
9.	29.992	29.970	29.60	30.00	29.93	29.80	29.76	54	40	44	55	42	46	43	ne.	n.	e.	0.06
10.	29.914	29.855	29.50	29.90	29.84	29.73	29.73	51	42	42	55	37 $\frac{1}{2}$	49	41	nw.	nne.	ne.	0.01
11.	30.101	29.837	29.50	29.89	29.98	29.77	29.82	56	30	47	54 $\frac{1}{2}$	41 $\frac{1}{2}$	48	44	n.	n.	e.	0.16
12.	30.205	30.172	29.80	30.09	29.99	29.87	29.82	62	41	52	59 $\frac{1}{2}$	38	46	43	e.	nne.	e.	0.05
13.	30.035	29.728	29.56	29.80	29.66	29.53	29.82	63	41	51	56 $\frac{1}{2}$	47	45	49	sw.	sw.	w.	0.02
14.	29.623	29.525	29.20	29.43	29.31	29.33	29.27	68	47	61	57	47 $\frac{1}{2}$	55	47	w.	s.	0.10
15.	29.571	29.538	29.06	29.31	29.40	29.35	29.27	69	49	56	58	50	49	48	nw.	sw.	ese.	0.02
16.	29.422	29.352	29.00	29.23	29.18	29.17	29.11	68	50	54	57	43	46	47	sw.	se.	se.	0.10
17.	29.376	29.286	28.80	29.80	29.10	28.93	28.86	62	49	61	62	48	49	46	sw.	sw.	nw.	0.13
18.	29.856	29.457	28.97	29.22	29.58	28.92	29.29	66	47	57	64	47	45	43	w.	sw.	n.	0.21
19.	30.000	29.935	29.44	29.80	29.82	29.57	29.68	66	50	51 $\frac{1}{2}$	64	40	46	44	sw.	se.	se.	0.24
20.	29.821	29.721	29.38	29.65	29.52	29.65	29.56	56	50	54	62	44	45	44	sw.	s.	se.	0.17
21.	29.836	29.771	29.38	29.62	29.62	29.50	29.71	71	47	60	63	48	45	47	e.	s.	sw.	0.30
22.	29.866	29.743	28.99	29.44	29.48	29.32	29.22	66	45	54	60	47	49	49	sw.	sw.	s.	0.05
23.	30.205	29.977	29.48	29.70	30.00	29.42	29.80	69	39	58	60	50	59	53	w.	sw.	sw.	0.20
24.	30.202	30.036	29.70	30.02	29.85	29.78	29.61	76	50	64	61 $\frac{1}{2}$	44	57	51	w.	w.	s.	0.05
25.	29.971	29.945	29.46	29.76	29.76	29.58	29.50	72	50	58	61 $\frac{1}{2}$	49	54	51	nw.	calm	sw.	0.15
26.	30.013	30.001	29.50	29.68	29.82	29.40	29.64	73	48	55	60	48 $\frac{1}{2}$	53	50	sw.	calm	sw.	0.22
27.	30.186	29.910	29.55	29.93	30.07	29.68	29.91	77	50	61 $\frac{1}{2}$	62	45	59	50	sw.	calm	sw.	0.04
28.	30.173	30.131	29.56	30.15	30.17	30.04	30.02	58	50	59	67	40	55	51	ne.	calm	sw.	0.22
29.	30.208	30.165	29.70	30.13	30.03	29.90	29.78	75	46	58	62	40	59	52	w.	sw.	sw.	0.07
30.	30.183	30.076	29.65	30.00	30.03	29.78	29.88	77	53	61	64	50	56	52	sw.	w.	sw.	0.21
31.	29.999	29.970	29.50	29.88	29.73	29.70	29.37	79	45	66 $\frac{1}{2}$	59	42	58	51	sw.	calm	sw.	0.22
Mean.	29.915	29.840	29.44	29.820	29.090	29.616	29.661	66.13	44.25	55.0	59.6	43.7	50.64	46.82	3.53	2.75	2.42	0.78

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[THIRD SERIES.]

AUGUST 1849.

XI. *On the Lignites and Altered Dolomites of the Island of Bute.* By JAMES BRYCE, Jun., M.A., F.G.S.*

I. *Introduction.*

1. **T**HE only account which we possess of the geology of Bute is that given by Dr. MacCulloch in his "Description of the Western Islands of Scotland." During the thirty years that have elapsed since the publication of that work, no observations that I am aware of have been put on record, either supplementary to this account or in correction of it. Indeed the island seems to have been entirely overlooked; the superior grandeur and interest of the sister isle of Arran having wholly absorbed the attention of geologists. Yet Bute has many points of great interest in itself; and phænomena which in Arran are but obscurely shown are here fully exhibited. During a residence in the island for a part of last summer, I had frequent opportunities of testing the accuracy of Dr. MacCulloch's account; and it is but justice to the memory of that distinguished geologist to say, that I have found the description of the phænomena to agree very closely with my own observations; and his work to be, here as well as in other places, a safe and pleasant guide.

II. *Corrections and Supplementary Remarks.*

2. The island of Bute is naturally divided into four portions, by three deep depressions or valleys, which traverse it perpendicularly to its greatest length. These low tracts terminate on either side of the island in deep bays, between which there can be no doubt, as well from the lowness of the ground as from the marine character of the materials of which these tracts are composed, the sea once flowed; thus forming

* Communicated by the Author.

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three straits or narrow channels dividing Bute into four islands. From measurements made by Robert Chambers, Esq., and given in his beautiful and interesting work on Ancient Sea-Margins, just published, it appears that these low tracts are about thirty feet above the present sea-level; an elevation very nearly the same as that of the flat region between Loch Lomond and the Clyde, in which we have the additional and more satisfactory evidence derived from shelly deposits.

Another interesting feature in the structure of Bute, and one intimately connected with the origin of the low tracts referred to above, is the terrace which surrounds almost the whole island, at the height of between twenty and thirty feet above high-water mark, and along which the road is conducted throughout the greater part of the coast. The cliffs, which in many parts rise above this terrace, are often worn into caves, and bear other obvious marks of the action of the sea. This terrace is no doubt the former beach. It is well-marked along the opposite coast from Gourock to Largs, in the Cumbrays, in Arran, and on most of the sea-lochs connected with the great estuary of the Clyde. Taking this along with other evidence accumulated by Mr. Smith of Jordan Hill in various papers, we cannot hesitate to admit, that at a comparatively recent period such a change of level has been effected in Bute as to convert a detached group of islands separated by narrow straits, into continuous land.

3. The valleys just described are the boundaries between dissimilar strata, the line of junction generally keeping the middle of the valley, but often concealed either by marshy ground or accumulations of shingle. The northern portion of the island, between the Kyles on one side and Kaimes and Ettrick bays on the other, is composed of mica-slate. The district south of this, bounded by the Rothsay Valley, presents siliceous and common clay-slate; while the portion extending from this valley to that of Kilchattan is occupied by red sandstone, usually conglomerate; and finally, the southern portion is composed of various rocks of the trap family erupted through the sandstone and overlying it. The connexion of these strata with the mainland is most intimate. The slate and sandstone are the terminal portions of those great bands of sedimentary strata which extend from Angus to the Clyde, being parallel throughout to the granitic axis of the Grampian chain; while the erupted rocks in the south of the island are a prolongation of the great outburst of the igneous formations, which, affecting a general parallelism with the same axis, stretches from sea to sea in considerable ranges, as the Kilpatrick and Campsie Hills, the Ochills and some minor ridges in the south-east

of Perthshire. The valleys intersecting the island seem obviously a part of that great system of parallel fractures which run in a north-east and south-west direction on both sides of the Grampians, and are probably due to the original upheaval of that chain, and partly to the subsequent eruption of the igneous rocks just mentioned through the old red sandstone, and the coal formation which rests upon it.

4. The strata of sandstone are fully exposed on the shore and in the cliffs from Rothsay to Ascog. South of Bogany Point limestone appears interstratified with the sandstone, the two rocks gradually passing into one another at the junction. Dr. MacCulloch describes one bed—I noticed several others; but the beds being thin, of small horizontal extent, and containing generally much siliceous matter, the rock is of little æconomical importance in this place. On the north side of the small rocky promontory south of Ascog Mill thin courses of nodular limestone traverse beds of brown shale. Subordinate to the sandstone, the shale is of considerable thickness, and appears again in the high banks above the road.

The south side of the same promontory presents the following strata:—The lowest bed is a fine-grained bluish-gray nodular limestone; it is succeeded by a black bituminous shale, containing veins of coal less than a quarter of an inch thick. A bed of concretionary limestone rests on the shale, the base or paste being a dark-coloured limestone, and the concretions rounded lumps of the same rock, often of considerable size. The upper part of the cliff is occupied by trap in various prismatic forms. By the contact of the trap, both the base itself and the imbedded lumps are so much altered as very closely to resemble the trap itself. The trap rock occupies a considerable area inland, and is upwards of 100 feet thick. Dr. MacCulloch has fallen into a slight error regarding it: he says, "When examined on the shore it appears rather to pass through the sandstone than to lie over it; but there is considerable obscurity in this place, as the lateral junction of the two is concealed by a cavity filled with earth." The junction is better exposed at present, probably in consequence of the continued action of the sea; and there can be no doubt that the relative position of the strata is such as above described. The trap reposes upon the sandstone, and does not pass through it.

5. The most considerable mass of limestone in the island occurs on the south side of Kilchattan Bay. Its characters are accurately described by Dr. MacCulloch, but he has mistaken its position. "This bed seems to lie over all the sandstone strata at this place, and to be the rock immediately in

contact with the superincumbent trap." A little way above the limestone quarry, near the ruins of Kelspoke Castle, the beds of sandstone are distinctly seen dipping towards the trap, both the dip and inclination being the same as below the limestone; and it thus appears that the limestone is here, as in other parts of the island, subordinate to the sandstone, and of cotemporaneous origin.

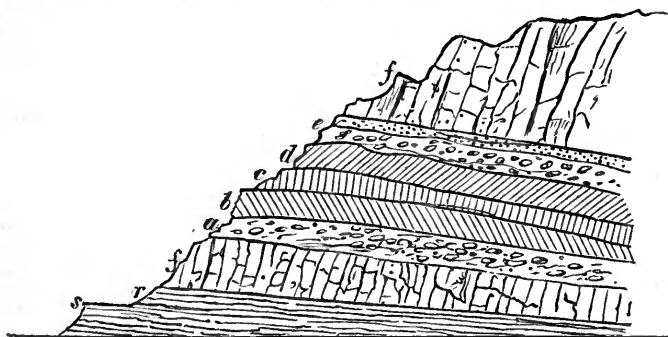
6. The limestone and shale which are interstratified with the sandstone in several places, and at Ascog are also accompanied by very thin veins of coal, bear a striking resemblance to true coal measures; and it is therefore not surprising that coal has been thought to exist in or under this sandstone, and that several attempts have been made to discover it. These, however, have been fruitless, and must always prove so; since there can be no doubt that the sandstone is the old red, and therefore inferior to the whole coal formation. The reasons for this opinion may be briefly stated.

(1.) Since in the adjoining tracts the series of rocks successively superimposed on the central granite is complete, and old red sandstone occupies in these its proper place, we may fairly infer that the sandstone which in Bute succeeds the slate series must be the old red. (2.) This sandstone, if continued on the line of its bearing, would coalesce with that which forms the Cumbrays, and with that which, rising to the west from beneath the great mineral basin of Ayrshire, skirts the coast from Ardrossan to Gourock, and from Toward Point to Dunoon, and appears again on crossing the Firth, in Dumbartonshire and Stirlingshire, forming the lower portions of the Kilpatrick and Campsie hills, and thus constituting a well-marked boundary between the coal basins of Lanarkshire and the primary ranges of the Highlands. (3.) The true coal formation associated with carboniferous limestone exists in Arran, separating distinctly the old red sandstone from the new. This old red sandstone of Arran incloses beds of limestone which are similar to those of Bute, and contain the same fossils as those limestones termed cornstones, which in England are subordinate to the old red system. Thus the red sandstone of Bute seems to be identified with the Devonian series of Arran and England. The evidence derived from fossils is unfortunately not applicable, as I was unable to find a trace of any organic substance either in the limestone, sandstone, or shale; and the same statement has been made by Dr. MacCulloch. It is probable, however, that organic remains will yet be found on a more extended and careful search.

III. The Lignite Beds.

7. The trap rocks at Ascog derive their chief interest from being the repository of beds of lignite; a substance so rare in Scotland, that I believe no well-marked beds occur on the mainland, and but few in the other islands; and these in situations very difficult of access. I was led to a careful examination of this carbonaceous deposit by the statement of Dr. MacCulloch, that some of the beds occurring here were unlike any he had seen in his survey of the western islands.

The principal bed is situated in the face of the cliffs above the road, a little to the south of Ascog Mill, as shown in the annexed section giving the various beds.



s, sandstone; *r*, terrace and road; *ff*, greenstone; *a*, trap-tuff; *b*, red ochre; *c*, lignite bed; *d*, pisolitic ochre; *e*, porphyritic amygdaloid, the upper portion much-altered.

The lowest bed resting on the sandstone is a small-grained rudely columnar greenstone; the junction is, however, concealed. Over this is a trap-tuff with a base of greenstone, and imbedded spherical lumps of the same substance. This is followed by a bed of red ochre of coarse texture, traversed by numerous black iron seams, which have doubtless been produced from a change in the oxidation of the component iron. Over this is the lignite bed: it is three feet thick, and consists of hard stony coal, interstratified with a yellowish-white shale, both being much intermixed with pyrites. The coal has been so much altered throughout its whole thickness by the contact of the trap rock, that Mr. Rose of Edinburgh, to whose examination I submitted the best specimens I could find, in order that he might determine the species of wood, but without mentioning the geological situation of the coal, was "unable to obtain a slice in consequence of the structure being altered by the contact of a whin dike." The coal has been

worked to some extent by driving an adit inwards on the line of the dip, which is about 20° to the westward; but the workings have been for some time abandoned, and the inner and lower portions are now full of water.

The floor of the coal has been already described. The roof is a peculiar rock. It consists of a base or paste of an ochreous steatite, with imbedded round pieces of the same substance, and may hence be called a pisolitic ochre; it is $3\frac{1}{2}$ yards thick. The bed above this is of the same character; but the base feels less unctuous, and with the imbedded steatite it contains also imbedded calcareous spar. The base effervesces briskly with an acid; and hence we may call the rock a calcareous amygdaloid. The upper portion of this bed, to the thickness of a few inches only, is very hard, and has a semivitreous appearance, and thus closely resembles a porphyry. In common with the trap above, and, indeed, all the beds in this locality, it contains much disseminated iron. The rest of the cliff is occupied by greenstone, which is the same as the lower bed resting on the sandstone.

Another bed of lignite occurs on the opposite, or north-west side of the trap district, overlooking Ascog Lake. The coal dips to the interior of the area, that is, nearly south. It is of about the same thickness, and is accompanied by beds of steatite and red ochre, very similar to those above described; but the nature of the ground is such that a complete section cannot be had, and the precise number, therefore, and order of the beds cannot be exactly stated. The association, however, of the lignite with ochres and steatites here also is sufficiently distinct, and it is even probable that these beds are persistent throughout the whole of this district. It is to these ochreous and steatitic beds that Dr. MacCulloch refers, when he says that he "has met with no similar substance among the numerous trap rocks examined in the course of the survey of the western islands." He has not indeed described any such strata; yet casual mention is made (vol. i. p. 376) of an iron-clay and jaspery substance, forming extensive beds in the trap of the cliffs of Talisker, in Skye—the same in which the lignite also occurs—and that these are often variegated with red, gray, and purple colours. No further description is given, nor is the precise position of the coal mentioned, the cliffs being very difficult of access.

But even by such a brief notice the steatitic beds and variegated ochres are easily recognised; and though these characters are not very distinctly marked in the beds we have been describing in Bute, yet they apply exactly to the red and variegated ochres, which occur as members of the trap series of

the north-east of Ireland. These rocks attain a much more complete development than in this country, both geographically and in relation to the number and variety of the beds. They extend continuously over an area of upwards of 1000 square miles; and while the thickness is, on an average, about 300 feet, in very many cases it reaches to 1100 or 1200 feet. The whole series reposes on the chalk formation, while the corresponding rocks of this country rest upon the old red and carboniferous systems. Now in this series the lignites occupy a determinate place; they occur in the middle region, associated with the steatites and variegated ochres, which are always largely developed wherever the series approaches to completeness. Instances may be seen at various points in the cliffs at the Giant's Causeway, at Ballintoy, at Glenarm, and at numerous places in the interior of the district. Similar beds are associated with the lignites of Bute and Skye, and most probably also of others of the Western Isles, though the notices are too vague to be relied on. We are thus led to the interesting conclusion, that such association is not accidental, but has been determined by the prevalence, over a considerable area, of certain similar and fixed conditions, regulating the succession of the igneous eruptions, the mode of their consolidation, and the periods of repose during which the productions of the adjoining dry land were swept down and entombed.

IV. *Altered Dolomites.*

8. The dikes of Bute are composed of greenstone or basalt, and are extremely numerous. They traverse the different strata in every possible direction, and are well seen upon the rocky parts of the coast. All the usual phænomena are remarkably well exhibited by them, and can be studied together in a small space. *Crystals of quartz* and other substances are developed in the shale beds near the contact. The dikes can in some instances be traced continuously for several miles, preserving the same direction, and the same width—two or more are sometimes seen to meet and coalesce for some distance and again to separate—a narrow dike branches off into several filaments, which unite again—portions of the rock which is traversed are frequently found entangled in the dike; and these, as well as the contiguous strata, present the usual alterations now universally acknowledged to be the result of igneous action.

A remarkable change is produced on the Kilchattan limestone by one of these dikes. Its direction is very nearly the same as the dip of the limestone, and the effects are well seen at

the eastern side of the quarry. Along the plane of contact with the dike the limestone is altered to the state of a granularsaccharine marble, which on the application of a slight pressure crumbles into a fine powder. This is succeeded by a hard crystalline marble, the crystals appearing in distinct flakes. Between this and the first change, which is one of simple induration, there are many gradations. Similar effects are common at the contact of limestone with plutonic rocks; in some localities they are accompanied by other singular changes of a chemical nature. Magnesia, and sometimes silica and alumina, are introduced into the composition of the limestone, so that simple carbonate of lime becomes a double carbonate of lime and magnesia. The question whence this magnesia has been derived has occasioned much difference of opinion among geologists. Some imagine that it has been transferred from the plutonic rock to the limestone; while others hold that, as fractures and dislocations of the earth's crust accompanied the eruption of these plutonic rocks, gaseous exhalations might find their way from beneath, and introduce carbonate of magnesia and other substances into rocks near the surface. In confirmation of this view, Mr. Phillips has shown in his *Geology of Yorkshire*, that "common limestone is dolomitized by the sides of faults and mineral veins far away from igneous rocks of any kind;" and some distinguished chemists have expressed their belief that carbonate of magnesia may be sublimed by the action of great heat. (Rep. Brit. Assoc. for 1835, Trans. Sect. p. 51; Phillips's *Geology*, vol. ii. p. 98.) Much doubt, however, still hangs about this subject. Cases occur in which magnesia has been introduced, although the limestone could not have been subject to such a pressure as would confine its carbonic acid when the rock was softened by heat.

Being anxious to elucidate, if possible, this obscure subject, I submitted two specimens of the rock to Mr. John Macadam, lecturer on chemistry in this city, for examination with reference to the presence or absence of magnesia. The following is Mr. Macadam's report; the specimen referred to as No. 1 is the saccharine marble from contact with the dike; No. 2 is the unaltered limestone—both were average specimens.

"I have carefully subjected to chemical analysis the specimen of limestone No. 1, with special reference to the presence or absence of magnesia; and I find from the indications given, that carbonate of magnesia constitutes about $2\frac{1}{2}$ per cent. of the whole mass. The mineral is not, therefore, a double carbonate of lime and magnesia. Its other and prin-

cipal ingredients are carbonic acid and lime, besides which silica is present, as also traces of oxide of iron and alumina.

"In the specimen No. 2 I find magnesia in great abundance, the amount present being equivalent to 33.72 per cent. of carbonate of magnesia. The other constituents present are similar to those reported in No. 1. From the large proportions of carbonate of lime and carbonate of magnesia present in specimen No. 2, it would appear to be a species of dolomite. It may be noticed that the physical characters of No. 2 are very different from those of No. 1; the former is difficult to pulverize, the latter is extremely susceptible of division.

"The action of strong hydrochloric acid on both specimens causes a portion of gelatinous silica to appear, showing the presence of a silicate, which may be that of magnesia, since the quantity of gelatinous silica is about sufficient to combine with the 1.28 per cent. of caustic magnesia existing in the specimen No. 1. There is a less quantity of this gelatinous silica in No. 2. The greater portion, however, of the silica present in both specimens remains undissolved in the gritty or pulverulent condition, and is hence in a state of mere mechanical mixture with the other constituents of the limestone. It would require a minute quantitative analysis to determine whether the 1.28 per cent. of magnesia exists as a carbonate or silicate, or partly as both."

The phenomena are thus of a contrary character to what I had anticipated; the unaltered rock is a dolomite, and contains nearly 34 per cent. of carbonate of magnesia, while the altered rock contains less than 3 per cent. What has become of the constituent magnesia? Has it been driven off by the heat to which the limestone was exposed? Most chemists are unwilling to admit that this is possible; and it may reasonably be objected, that if the limestone had been exposed to so high a temperature as to vaporize its magnesia, the silica would not be mechanically present, but would have entered into chemical combination with the lime or the magnesia, and have formed a silicate.

That whin dikes have sometimes been the means of producing such a combination has been shown by an eminent chemist. In a valuable paper by Dr. Apjohn on the Dolomites of Ireland, published in the *Dublin Geological Journal*, vol. i., the details of an analysis of the white chalk of Antrim, altered to the state of a saccharine marble, are given (p. 376); and it is remarked in conclusion, that "the stone under consideration consists of silica, combined with the mixed oxides of calcium, magnesium and iron (the carbonate of lime being mechanically present), and is therefore a mixture

of trisilicates, very analogous in its composition to olivine. We are thus enabled to understand why olivine should be so very frequently found in trap rocks, and to refer its origin to the contact of silex at a high temperature with an excess of the basic oxides; and we have in some degree a demonstration that the dolomites which contain siliceous sand could not have been exposed at any time to a heat sufficiently high to account for the introduction into them of magnesia in the vaporous state; for by such a heat a silicate of lime or magnesia, or of both, would have been produced."

The presence of these silicates in both our specimens is shown by the gelatinous silica appearing; yet a greater quantity of silica is present mechanically, which, as already stated, seems inconsistent with the exposure of the rock to intense heat; unless, indeed, we could suppose that the silica has been introduced by infiltration, or the magnesia removed by the solvent power of free carbonic acid at a period subsequent to the consolidation of the dike from a state of igneous fusion. But it is needless to pursue the subject further with our present limited knowledge of facts.

Postscript.—Altered Dolomites.

The preceding account was drawn up some months ago for insertion in the *Philosophical Magazine*. On consideration, however, it was thought best to withhold it until careful *quantitative* analyses of the limestones should be obtained. This has now been accomplished through the kindness of Dr. Robert D. Thomson, of Glasgow University, of whose skill and competence it would be presumption in me to speak. It is hoped that these will afford definite terms of comparison with other analyses, such as those of Dr. Apjohn already referred to; and that their publication may lead to the formation of clearer views respecting an obscure question in theoretical geology.

The analyses kindly furnished me by Dr. Robert D. Thomson are as follows:—

Specimen No. 1 is the saccharine marble from contact with the dike at Kilchattan,—in the highest state of alteration.

No. 2 is the hard crystalline marble, having the crystals in distinct flakes, more remote and less altered than No. 1.

No. 3 is the unaltered limestone from the middle of the quarry, remote from the dike,—an average specimen.

No. 4 is the altered limestone from contact with the overlying trap at Ascog Mill; it is an impure dark-coloured rock of an earthy aspect, and very like the trap which rests upon it.

No. 1. Spec. grav. 2·710.

By Mr. J. H. Turnbull. By Mr. Henry S. Thomson.

		I.	II.
Silica	} 6·91	5·16	5·70
Alumina			
Protoxide of iron . . .	1·68	1·50	1·28
Carbonate of lime . . .	90·65	...	91·08
Carbonate of magnesia .	1·00	...	1·17
	<hr/> 100·24		<hr/> 99·23

No. 2. Spec. grav. 2·570.

		I.	II.
Silica	} 1·94	0·28	0·28
Alumina			
Protoxide of iron . . .	0·52	...	0·56
Carbonate of lime . . .	96·48	98·76	96·58
Carbonate of magnesia .	1·23	...	2·24
	<hr/> 100·17		<hr/> 99·66

No. 3. Spec. grav. 2·679.

	I.	II.
Silica	} 9·70	9·08
Alumina		
Protoxide of iron . . .	1·12	1·12
Carbonate of lime . . .	67·42	72·12
Carbonate of magnesia .	17·31	...
Water, coaly matter and carbonic acid }	4·45	4·74
	<hr/> 100·00	<hr/> 100·00

No. 4.

Silica	} 64·21	64·46
Alumina		
Protoxide of iron . . .	6·42	6·60
Carbonate of lime . . .	24·00	21·20
Carbonate of magnesia .	4·62	2·85
Water and carbonic acid .	1·75	4·89
	<hr/> 100·00	<hr/> 100·00

The silica present is in a state of mechanical mixture.

These analyses are confirmatory of the main points of the views already stated, and seem clearly to establish the new and remarkable fact, that by the igneous action in these instances the magnesia has been driven off from the limestone.

The unaltered rock is a dolomite containing nearly 70 per cent. of carbonate of lime, and nearly 20 per cent. of carbonate of magnesia; while the altered rock contains but from 1 to 2 per cent. of the latter ingredient. To what cause are we to assign the changes that have taken place? Has the magnesia been sublimed by heat? or has it been withdrawn by the solvent power of free carbonic acid? On the nature of these and the other chemical changes that have been induced, I do not feel myself competent to express an opinion; and from such limited premises it would be unphilosophical to draw any general conclusions. The subject is one, however, of great interest both to the geologist and chemist, as the facts are directly opposed to the received views*; and as no instance of similar changes on dolomitic rocks has, so far as I am aware, ever been put on record.

High School of Glasgow,
June 1849.

XII. Notices. By Prof. J. D. FORBES†.

1. On an Instrument for measuring the Extensibility of Elastic Solids.

THIS instrument is almost a faithful reproduction of S'Gravesande's apparatus described in his *Physices Elementa Mathematica*, 1742 (but not in the previous editions). It is described or alluded to by few modern writers, except Biot in his *Traité de Physique*. It consists of a strong wooden table or frame, with a vice at each end, between which a wire or lamina may be stretched with a determinate tension by means of a weight attached by a cord, passing over a pulley in the manner of the musical apparatus called a monochord. After the tension is adjusted both vices are screwed fast, the space included between them being exactly fifty inches. If, now, any deviation of the middle point of the wire included by the vices be made (similar to the action of sounding a harp-string), the force required to pull it a certain distance aside will depend,—1st, on the length of the wire; 2nd, on its tension; 3rd, on its extensibility, or the modulus of elasticity.

S'Gravesande employed his apparatus to verify Hooke's law, that the extension is as the extending force within the

* For the latest and best account of the views of geologists on this subject see the well-known work of Dr. Daubeny on *Volcanos*, 2nd edit., recently published; also an admirable *résumé* in the eloquent address of Prof. Oldham of Dublin University, which has just appeared.

† From the Proceedings of the Royal Society of Edinburgh.

limits of perfect elasticity. But it does not seem to have occurred to him, nor (singularly enough) to later experimenters, to deduce from the forces required to produce given deviations, the specific extensibility, or what Dr. Young calls the *modulus of elasticity* of the body.

It is essential that the deviation from the rectilinear position of the wire should be ascertained with great nicety, and S'Gravesande's contrivance effects this in a very neat and satisfactory way. A fine steel chain attaches by a hook to the middle point of the extended wire, the other end being secured to the circumference of a nicely-centred wheel. Another chain attached similarly to the wire of the same wheel has a scale attached to it, a weight placed in which causes the wheel to revolve, and by means of the first chain and hook pulls the wire out of the straight line. A long index fixed to the same axis with the wheel points out the deviation on a much-magnified scale, referred to a divided semicircle of brass. Thus a weight being placed in the scale, the corresponding deviation is instantly shown.

Let P be the weight in the scale, D the deviation of the wire, s = half the length of the wire between the vices; it was proved in this communication that

$$P = 2T \frac{D}{s} + M \left(\frac{D}{s} \right)^3,$$

where M is the *modulus of elasticity*, measured in grains, which is easily reduced to the equivalent length of a similar wire or lamina, according to Dr. Young's definition. This is on the supposition that Hooke's law of elasticity (the extension is as the extending force) is correct; and that law is verified if the term $M \left(\frac{D}{s} \right)^3$ be found for the same wire practically to vary as the cube of the deviation. The value of M , the modulus, is also at once given by a single observed deviation, the tension being known.

A small correction in the value of P is to be made for the weight of the wire deflecting it from a straight line. This small correction had not escaped the notice of S'Gravesande when he verified Hooke's law.

Example. A steel pianoforte wire, tension 50,000 grains = T ; $s = 25$ inches.

Values of D .	Values of P .	$2T \frac{D}{s}$.	$M \left(\frac{D}{s} \right)^3$.
.25 inch	1100 grains	1000	100
.50 ...	2720 ...	2000	720
.75 ...	5400 ...	3000	2400

94 *On the Refractive and Dispersive Power of Chloroform.*

The numbers in the last column should vary as the cubes of those in the first, or be as 1, 8, 27. If we deduct 10 grains from each of them for the action of the weight of the wire depressing it, we shall have these numbers—

90 710 2390 ;

dividing respectively by 1, 8, and 27,—

90 89 88·5.

Hence the mean result for $M\left(\frac{D}{s}\right)^3$, for $D = \cdot 25$, or $\frac{D}{s} = \frac{1}{100}$, is nearly 89 grains, consequently,

$$\frac{M}{(100)^3} = 89,$$

and $M = 89,000,000$ grains.

But a foot in length of the wire in question weighs 11 grains. The equivalent modulus of elasticity is therefore very nearly 8,000,000 feet of the wire in question, which agrees closely with the received numbers for steel wire.

2. *Note respecting the Refractive and Dispersive Power of Chloroform.*

From an experiment made in very cloudy weather, and therefore rather unfavourable light, I determined the following indices of refraction for pure chloroform, prepared by Dr. George Wilson, of sp. gr. 1·4966.

The measure of the refracting angle of the prism was $39^\circ 41'$. References were made to the principal lines of the spectrum, as below: the temperature of the fluid was probably 54° .

Extreme red	$\mu = 1\cdot4475$
B (in the red)	1·4488
D (in the orange-yellow)	1·451
b (in the green)	1·456
F (in the blue)	1·457
H (in the violet, being the least refrangible of the two groups so designated) }	1·463
Extreme violet	1·4675

Hence the refractive index is by no means remarkably great, being nearly that of wax, spermaceti, and several of the essential oils.

The dispersive power, or $\frac{\delta\mu}{\mu-1}$, is equal to ·045, which again agrees nearly with that of the essential oils. The high specific gravity of the body appears to have no marked influence in increasing its action on light.

3. *Note regarding an Experiment suggested by Professor Robison.*

In his memoir of Dr. Chalmers, lately read to this Society, Mr. Ramsay has referred to an experiment which Dr. Chalmers was anxious to have performed on the tide-wave in the Bay of Fundy. The object was to determine the earth's density by the attraction of the tide-wave on a plummet or spirit-level, on the same principle as Maskelyne's experiment on Schiehallion, but with the superior advantages arising from the perfect homogeneity of the attracting mass, and from the circumstance that all the observations might be made at a single station. The experiment might, in short, appear to unite the advantages both of Maskelyne's and Cavendish's methods of determining the earth's density.

The suggestion was Dr. Robison's, and Dr. Chalmers had it from him. It is contained in the *Elements of Mechanical Philosophy*, edit. 1804, page 339, and is given in the following words:—"Perhaps a very sensible effect might be observed at Annapolis-Royal in Nova Scotia, from the vast addition of matter brought on the coast twice every day by the tides. The water rises there above 100 feet at spring-tide. If a leaden pipe a few hundred feet long were laid on the level beach at right angles with the coast, and a glass pipe set upright at each end, and the whole filled with water, the water will rise at the outer end, and sink at the end next the land as the tide rises. Such an alternate change of level would give the most satisfactory evidence. Perhaps the effect might be sensible on a very long plummet, or even a nice spirit-level."

It is needless to observe that the methods proposed by Dr. Robison are not the best which might be suggested; but that, in consequence of the extreme simplicity of the observation, considered as a purely astronomical one, a deviation of the direction of gravity of only a very few seconds could be ascertained within small limits of error*.

I thought it worth while to make the calculation approximately for an assumed height of the tide-wave. Had the result been at all encouraging, I should have taken pains to ascertain, on good authority, the exact rise of the tide, and the circumstances of the locality whence the rise is greatest.

* The micrometric observation of a plumb-line, as in a zenith sector, would be sufficient; or, as Professor Smyth has suggested to me, the view of the wires of a transit-instrument, with a collimating eye-piece, as reflected in a mercury trough,—an observation, the accuracy of which may, he states, be brought within one-twentieth of a second.

I have calculated the horizontal attraction of a semicylinder of water 100 feet thick, and of about two, four, and eight miles radius upon a point at the extremity of the axis of such a semicylinder; because these conditions can easily be reduced to calculation, and because they represent very approximately the circumstances of an attracted point placed at high water-mark on a vertical sea-wall facing a basin or estuary. The radius of the attracting mass of water being represented (more accurately) by 10,000, 20,000, and 40,000 feet, I find the influence of a tide-wave 100 feet thick upon a plumb-line to produce a deviation of only $0''\cdot44$ (forty-four hundredths of a second), $0''\cdot50$, and $0''\cdot53$; the effect increasing extremely slowly with the radius, as might be expected. If the tide rose only fifty feet, the first effect would be reduced to $0''\cdot246$.

Even the greatest of these calculated deviations affords no ground for hoping that the method of Robison could be applied with any success to determine the earth's density.

It is rather singular that this ingenious suggestion is not once alluded to, so far as I am aware, by any writer on the figure and density of the earth; yet surely it was as worthy of notice as Dr. Hutton's proposal to measure the attraction of an Egyptian pyramid. (Phil. Trans. 1821.)

XIII. On a Simple Apparatus for Washing Precipitates.

By EUSTACE C. SUMMERS*.

MY attention has been lately drawn to an account by M. Bloch, given in the May Number of the *Annales de Chimie et de Physique*, vol. xxvi. p. 126, 3rd series, of a new method of washing precipitates by means of a self-regulating siphon. The instrument proposed by M. Bloch is, however, open to several serious objections. In the first place, as soon as the requisite quantity of water has been supplied to the filter, the water rises in the exterior air-tube to the level of the liquid in the vessel from which the supply is obtained. Now if the precipitate be very light, part of *it* also is liable to gain access to the tube and adhere to its sides. Again, when the level of the water in the filter falls, the column of water in the air-tube does *not* always *fall* so as to leave free access to the air. On the contrary, the whole or part of it is frequently carried upwards by the pressure of the air, and falls, not into the filter, but into the reservoir; and should any of the precipitate be carried with it, which is far from improbable, of course an analysis might be at once invalidated. I say

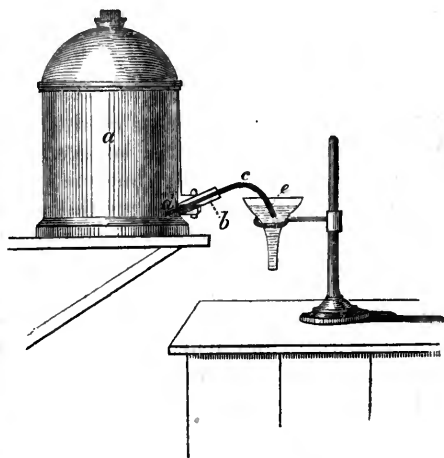
* Communicated by the Author.

nothing of the difficulty, which, however, is by no means slight, of bending the two concentric tubes; because all the advantages, together with greater convenience for washing, &c., may be obtained by having the tubes entirely distinct.

The apparatus of M. Gay-Lussac, as described in Mohr and Redwood's Pharmacy, is far superior to that now proposed by M. Bloch, inasmuch as the air-tube does not communicate with the fluid in the filter. The following contrivance has, however, occurred to me as furnishing the most simple and effectual way of attaining the object in view, and has the advantage of being applicable upon any scale of magnitude.

It consists of a reservoir, fig. *a*, having the mouth placed at its base. This, when

the vessel is filled with water, is closed by a cork, pierced by a straight tube *b*, passing through it obliquely downwards. It is now obvious that the water will rise in the tube *b* so long as air can gain access to the interior of the reservoir; which, however, is prevented when the water in the tube is sufficiently high to cover the whole of its lower ex-



treimity. A siphon is then introduced through this tube so as to project into the reservoir slightly beyond the lower extremity of the tube, care being taken to leave sufficient space to allow of the passage of air between the siphon and the surrounding tube. The funnel is placed beneath the long leg of the siphon, so that the edge of the filter is a little above the level of the water in the straight tube. In this condition the apparatus works. As long as the level of the water in the filter is lower than the level of that in the tube *b*, the column of water *ce* will be heavier than the column *cd*, and consequently the siphon will keep up a supply of water, bubbles of air obtaining access to the interior of the reservoir, as the level of the water in the tube *b* is reduced by the action of the siphon. But no sooner does the water in the filter attain the same level with that in the tube, than the column of water *cd* is equal in pressure to the column *ce*, and consequently the action

of the siphon is stopped, the water in the tube *b* remaining at just sufficient height to exclude the air from the interior of the reservoir.

This simple apparatus may be affixed to *any* convenient vessel having an aperture at the bottom; all other openings being of course closed, so as to prevent the ingress of air except by the tube *b*. A common salt glaze earthenware washing-bottle, or a Griffin's gas-holder, will be found to answer the purpose perfectly.

Glasgow College Laboratory,
July 6, 1849.

XIV. *On the Sources of the Nile ; being an attempt to assign the limits of the Basin of that River.* By CHARLES T. BEKE, Esq., Ph.D., F.S.A. &c.*

IN treating of the sources of the Nile, it is not intended to regard any particular source or spring as being more especially and exclusively the true head of that river. It will not even be discussed whether this or that large branch of the river should be considered to be the principal one. Such questions at the outset are not likely to lead to any satisfactory result. Our object must be in the first place to determine the entire limits of the basin of the river; we have next to ascertain what principal arms unite to form the main stream; we must then trace to their heads the several smaller streams which form those branches; and when we have succeeded in all these points, we shall then—but not before—be competent to decide which of these numerous ramifications has the fairest claim to be regarded as the true source of the Nile.

It may however be objected, and not without a show of reason, that though this is the theoretical method of investigating the subject, yet the question is susceptible of a more practical solution;—that, as in the case of most other rivers, there is some particular stream which by common consent is looked on as *the source* of the Nile, and this without regard to its being actually the head of the longest or the largest branch. Were this really the case, we should in one sense be bound to acquiesce in the recognition of such a source, even though it might not possess all the attributes of the true head of the river. But the fact is not so. The Abáí, the river of Abessinia, whose source was visited and described by the Portuguese Jesuits in the beginning of the seventeenth

* Communicated by the Syro-Egyptian Society of London, having been read before that Society on the 9th of January 1849.

century, and 150 years later by Bruce, is (as I know from personal experience) undoubtedly considered by the modern Abessinians to be the Nile. But among the ancient Abessinians, (or more properly speaking the Axomites of Northern Abessinia, who were known to the civilized world in the first ages of the Christian æra,) it was the *Tákkazie*, and not the Abái, which was regarded as the Nile. Of this a direct proof is given in the second Adulitic inscription, which, recording the victories of one of the princes of that country, says:—"After this, I reduced Ava and Tzíamo, Gambêla and the country round it, Zingabênè, Angabè, Tíama, and the A'thagai, Kálaa, and *Semênè, a nation beyond the Nile*, among mountains difficult of access and covered with snow; in all this region there is hail and frost, and snow so deep that the troops sunk up to their knees. I passed *the river*, and subdued them:"—in which passage it is manifest that reference is made to the well-known province of Sámien *beyond the Tákkazie*, in which province are the loftiest mountains of Abessinia topped with snow, or more properly speaking, a light kind of hail*. It is not unimportant to the present question to remark that in the ancient Ethiopic language, the word *Tákkazie* is not a proper name, but an appellative signifying *river* in general; so that "the *Tákkazie*" is simply "*the river*;" and it is in this sense that in the Ethiopic Scriptures it is said that the waters of the *Tákkazie* were turned by Moses into blood.

But the question of superiority is far from lying between these two rivers, the *Tákkazie* and Abái, alone. Proceeding further southwards up the direct stream of the Bahr el Abyad or White River, we come, in about 9° N. lat., to another large arm named Sobát, Telfi, or River of Habesh, which in its upper course is called Bako, then Uma, and afterwards Gódjeb. The source of the Gódjeb is in Kaffa, where it is revered by the natives as the head of the Nile. Nevertheless there is no more real ground for such a preference of the Gódjeb than there exists in the case of the two other rivers already mentioned. The recent expeditions sent by Mohammed Ali, Pasha of Egypt, to explore the Nile, have ascended the White River

* "Snow is very rare in Abessinia; it is seen on days when the clouds are but little raised above the earth, and are at the same time widely extended. The flakes are small, triangular, and radiated.

"Hailstones which fall at an altitude of 4650 to 4700 mètres [15,500 to 15,600 English feet] have the form of a truncated polyedric pyramid, hollow. The edges of the pyramid are granulated. This hail is almost as light as at the base and summit; the hollow being in the form of a reversed cone. snow."—Letter from M. Schimper, on the Meteorology of Abessinia, in the *Comptes Rendus*, vol. xxvi. No. 7. (Feb. 14, 1848), p. 229.

as far as the country of Bari in 4° N. lat., 500 miles above the confluence of the Sobát or River of Habesh; and still the head of the Nile is there said to be distant a month's journey further *to the south*.

In the absence, then, of unanimity among the people of the countries watered by the several branches of the Nile, who each regard, not unnaturally, their own particular river as the principal stream; it is manifest that the question is an open one, and that its solution must be attempted on general and scientific principles.

When we trace the course of a river, from its mouth upwards, to the furthest extremity of each of its several tributaries, we arrive ultimately at a point where the waters no longer flow towards that river, but take their course in the opposite direction. This is called in Latin *divortia aquarum*—the parting (or flowing in contrary directions) of the waters—and in German *die Wasserscheide*, which means the same. English geographers have of late years adopted, through the geologists, the expression *watershed*, which is evidently a mere corruption of the German *Wasserscheide*, and was probably first brought into use among us by miners from Germany. But such an expression is objectionable; as to the mere English scholar it would seem to be a native compound of the words *water* and *shed*, and hence might give rise to the idea of a river's *basin* being intended rather than the division between two adjoining basins. The word *waterparting* is far preferable. For it is a literal English translation of the Latin *divortia aquarum* and of the German *Wasserscheide*; it expresses the true sense, and it is not susceptible of ambiguity.

The waterparting between the basins of two rivers is not necessarily coincident with a mountain-chain, or identical with it. On the contrary, we have instances of a river's passing through a mountain-chain, and taking its rise in an extensive elevated plain, so level in appearance as to render it difficult, at first sight, to determine in which direction the waters flow, or ought to flow. But though the waterparting may not be identical with an abnormal elevation, such as a mountain-chain, it must, in the nature of things, be the greatest *normal* elevation: it must be that point—or rather that line or series of points—at which the waters from the heavens, no longer continuing to find a passage in the one direction, seek it in another. Thus the waterparting forms the extreme limits of the basin of a river, and the boundary-line between the several distinct hydrographical systems of which a continent is composed.

The sources of the Nile, then, are all those head-streams

which rise along the extreme limits of the basin of that river, at the waterparting between it and the conterminous basins of other African rivers flowing towards the Red Sea, the Indian Ocean, the Atlantic and the Mediterranean respectively, or possessing (as is the case with some rivers of Asia and America, and even of Africa) separate inland hydrographical systems unconnected with the ocean. It is in this sense that I propose to treat of the sources of the Nile.

Commencing at the isthmus of Suéz, and proceeding southwards to about the 16th parallel of north latitude, we find a tract of desert country lying between the Nile and the Red Sea, and presenting no point of very great elevation. As, however, we approach the parallel of 16° N. lat., the land rises considerably, and forms the plateau or table-land of Abessinia. It is only of late years that the true character of this elevated region has been determined. Formerly it was generally considered that Abessinia consisted of a *succession of terraces* rising one above the other, the lowest being toward the Red Sea and the highest being in Enárea, where the waterparting between the Nile and the rivers having their courses towards the Indian Ocean was supposed to exist. Recent explorations have, however, determined that this opinion is erroneous. Instead of the country's rising in terraces as it recedes from the coast, its summit or culminating line is toward the coast itself, whence the land falls gradually towards the interior. This is best shown in the two sections of Abessinia, the one being from north to south and the other from east to west, published in the fourteenth volume of the Journal of the Royal Geographical Society of London.

In the beginning of the year 1841 I explored the waterparting from Ankóbar, the capital of Shoa, northwards about forty miles, as far as Gudem; and in the printed account of this excursion* I remarked, that "as the longitude of the waterparting in that direction [about 40° E. long.] corresponds very nearly with that of the waterparting in Northern Abessinia, it may perhaps be not unreasonable to infer that they are both formed by a continuation of *the same* central high land." This was before I was aware of the results of Dr. Rüppell's labours, and before I had carried my own series of levels further across the country to the west of Ankóbar. The comparison of these has not only proved the correctness of my surmise, but, combined with the personal explorations of Dr. Krapf and M. Lefebvre along the waterparting between Shoa and Tigre, and with what we know, though only more indefinitely, of its continuation to the south of Abessinia, it

* Journal of the Royal Geographical Society, vol. xii. p. 99.

enables us to arrive at tolerably satisfactory conclusions respecting the general character of the table-land of Eastern Africa.

As a whole, this table-land may be described as a succession of undulating plains, declining very gradually towards the west and north-west, irregularly studded with loftier mountain-masses, and intersected by numerous streams; which streams, after a short course on the level of the plateau, fall abruptly into deep-cut valleys, in which they soon attain a depression of from 3000 to 4000 feet below the general level of the table-land. The valleys of the larger streams, though at first mere fissures in the rocks, soon open to a considerable width: that of the Abái, in the south of the peninsula of Godjam, is at least twenty-five miles from the extreme points where it breaks from the table-land on either side. And, as the country within these valleys is exceedingly wild and irregular, possessing all the characters of a mountainous one; nothing is easier for a traveller, who has not first taken a comprehensive view of the entire region, and who, on crossing a river, finds himself shut up within a mass of broken country rising around him on all sides to a relative elevation of 3000 or 4000 feet, or even more, —than to suppose that, in ascending this broken country on either side, he is crossing a mountain-chain; whereas, on reaching the summit, he has merely arrived on the level of the table-land.

The fall of the rivers within their deep-cut valleys diminishes gradually as they flow north-westwards to join the main stream of the Nile; which latter, skirting the western flank of the high land, is the *sink* into which the Tákkazie, the Bahr el Azrek or Blue River, the Godjeb, Sobát or River of Habesh, the Shoaberri, and whatever other rivers there may be, are received; its current being very sluggish, and almost stagnant in the upper part of its course, except during the floods. In the dry season its bed would indeed almost seem to consist of a succession of lakes and swamps, rather than to be the channel of a running stream. At Khartúm, at the confluence of the Blue River with the Nile, the bed of the united stream is only 1525 feet above the ocean; and it is far from improbable that even as high up as the 5th parallel of N. latitude its absolute elevation does not much exceed 2000 feet.

On the seaward or eastern side of the waterparting, the declivity being much more abrupt and the extent of country much more limited than on the western side, the rivers must necessarily be of secondary importance. Thus, proceeding from the north, we do not meet with a stream deserving of name till we come to the Hawásh; and even this river is, near

Aussa, lost in lake Abhébbad before reaching the ocean. The Doho, Wábbi, or Haines's River, which is the next in succession, appears in like manner not to have sufficient water to reach the sea; at least not at all times of the year. Further to the south, we find the Wábbi-Giwéyna, or Gowín, or Juba River, possessing a substantive character as an ocean stream; but this river, during the dry season, has at its mouth a depth of only two feet. At a short distance to the south of the Equator is the Ozi or Maro, which river, though said to be of great extent, has very little water at the entrance. A short distance to the south of the Ozi is the Sabáki, which enters the sea near Melindah, and has a breadth of sixty yards at its mouth; but inland it is said to be very much larger. Yet further to the south, in about 8° S. lat., we find the Lufidgi, a considerable river, of which the Kwavi, in about 9° S. lat., appears to be one of the mouths; and beyond this, in between 10° and 11° S. lat., is the Livúma, also a large stream.

These rivers of the coast have been enumerated for three reasons:—1st. They all of them—as far at least as the Lufidgi—diminish very considerably in size as they approach the sea; which is a proof that they have not a sufficient supply of water to keep their channels open during the dry season: from which it also follows that the area of their respective basins cannot be very large. 2nd. They are all said to rise in the mountains of *Abessinia*; an expression which must be explained as meaning, not merely the “*Abessinia*” of Europeans, but the entire elevated land of Eastern Africa, which is known to the Arabs by the name of *Habesh*, and to the people of Sennár by that of Makádah. And thirdly, all these rivers are stated to communicate either with the Nile, or with the great lake called Nyassi, said to exist in the interior. This last statement, if not explained, might lead into error; and there is no doubt that a similar assertion with respect to other rivers of Africa—especially as regards the alleged union of the Niger with the Nile—has been the cause of much confusion and misunderstanding in the hydrography of that continent. Whatever may be the meaning attached to such an expression among Europeans, in the native phraseology of Africans it is merely the enunciation of the fact, that the several *sources* of these rivers of the coast are at the waterparting between their basins and those of the Nile and the streams draining into Nyassi; the contiguity of the respective sources being, according to the native mode of thinking, equivalent to an actual water-communication between the streams themselves.

To some persons it may hardly seem possible that such a mistake should occur. I will therefore give an example from

my own personal experience. In August 1842, when at the town of Yaush in Godjam, I obtained a good deal of valuable information respecting the countries lying to the east of the Abái and to the north of Shoa, from an intelligent Christian Abessinian merchant named Négaderas* Fanta, who traded between the market of Baso in Godjam and those of Warrahémano and Warrakallu in Eastern Abessinia.

On my inquiring of Fanta as to the course of the Milli and Berkonna, two rivers of Eastern Abessinia both tributary to the Hawásh, he answered that he knew them well, and that *they joined the Abái*. As I was well-aware that this was not and could not be the case, I began to fear that all the other information with which my friend had been supplying me might be of a similar character. But a little explanation showed me that he was quite right—that is to say, *according to his own way of thinking and speaking*. On my expressing myself doubtfully as to the correctness of his assertion, he at once appealed to *myself* as an eye-witness of the fact. “Do you not say (asked he) that you came to Shoa by the way of the Adál country?” I admitted that I did. “Consequently you crossed the Hawásh, into which river the Milli and Berkonna flow.” This, too, I could not deny. “The Hawásh, after passing between Adál and Shoa, runs round to the south of the latter country, between it and Gurágie. Does it not?” As I now began to conceive Fanta’s meaning, I did not think it worth while to dispute the correctness of this assertion; though I knew the fact to be that the course of the river in question is *from* and not *to* the south. “Well, then, the Hawásh joins the Muger, the confluence of which river with the Abai you have seen with your own eyes, if I mistake not.” This last was true enough. And so the good man, Fanta, by merely making the Hawásh run the wrong way, and regarding the Muger as the continuation of it, *because the two rivers have some of their sources in close contiguity on Mount Salála*, succeeded in demonstrating, to his own if not to my entire satisfaction, that the Milli joins the Abai! The alleged communication between the Kwara or Niger and the Nile, is doubtless to be understood in a similar manner; the sources of the eastern arm of the former and those of the western arm of the latter being at the waterparting between the respective basins of the two rivers, and thus “communicating with one another,” according to the ordinary African phraseology.

* *Négaderas* means literally *the head (ras) of the merchants* (negade), equivalent perhaps in its primary application and value to the *Console de’ Mercanti* of the Levant—whence our modern title of consul is derived. But it is now given as a complimentary appellation to most traders of standing.

How far the high table-land of Eastern Africa extends southwards it is impossible to determine in the existing imperfect state of our knowledge on the subject. But it would seem that we may safely trace its continuation beyond the Equator. The country of Mono-Moézi, which lies to the south of 2° S. lat., is described as an elevated plain, the ascent to which lies chiefly in the territories of the M'sagára and Woháha tribes occupying the low lands to the north-west of Zanzibar, which elevated plain may be regarded as a continuation of the Abessinian plateau. And, indeed, what knowledge we possess of the geography of Southern Africa leads to the inference that the same high land extends along the entire eastern side of the Continent as far as its southern extremity; its higher eastern edge and waterparting being distant from the Indian Ocean much less than from the South Atlantic, whereby the rivers all along the eastern coast are far less considerable than those of the western coast.

We have not the means of deciding how far this great waterparting of Southern Africa continues to form the boundary of the basin of the Nile. It is manifest, however, that in proceeding along it from north to south, we must at length reach a point where the waters on its western side, instead of continuing to flow northwards into the Nile, take their course westwards across the Continent. Consequently, beyond this point the waterparting of Southern Africa is between the hydrographical systems of the Indian Ocean and the Southern Atlantic; while at the same time a new waterparting comes into existence between the hydrographical system of the Southern Atlantic, as represented by the river Congo, and that of the Mediterranean, as represented by the Nile; which latter waterparting may, in a general way, be considered to run from east to west, or perhaps rather from south-east to north-west.

In the ordinary maps of Africa we find a chain of mountains laid down as stretching across the Continent from west to east, between the 7th and 8th parallels of north latitude. This chain bears the name of the "Mountains of the Moon;" it is supposed to be a continuation of the Kong Mountains of Western Africa, in which the rivers Senegal, Gambia, and Joliba or Niger, have their sources; and on its northern flank the sources of the Nile are also supposed to be situated.

The recent Egyptian expeditions to explore the Nile have, however, demonstrated that such a mountain-chain does not exist; for they have sailed over the alleged site of these "Mountains of the Moon," and have advanced as far as the country of Bari, in the 4th parallel of north latitude, without meeting with

any mountains that could lay claim to this title. A detailed account of the second of these expeditions, being the one which penetrated the furthest, has recently been published by M. Ferdinand Werne of Berlin, who took part in it*. The author states that, according to Lakono, king of Bari, the course of the river continues thence southwards a distance of thirty days' journey, as far as the country of Anyán, where it divides into four small branches. It may not be easy to determine at what rate this distance of thirty days' journey is to be calculated; but if we roughly estimate it at twelve miles per diem (merely as a first approximation), we shall have a distance of 360 miles, or six degrees of latitude; which distance, measured from Bari, carries us to two degrees south of the equator.

This brings us into the country of Mono-Moézi, of which mention has already been made; and this (coupled with various other circumstances which it is unnecessary to enter on here†) affords not only a plausible derivation of the name of the "Mountains of the Moon," in which the Nile is, by universal consent, considered to have its origin, but it serves at the same time to determine the position of those mountains in a manner that would seem to be but little, if at all, removed from the truth.

The "Mountains of the Moon" are an established feature of African geography. All writers, whether Arabian or European, mention them; all travellers in central Africa hear of them; and yet so indefinite, so various, so contradictory are the statements respecting these mountains, that nothing in the least degree satisfactory has been decided about them. There is however good reason for believing, that all that has been written and said on the subject of the sources of the Nile in the Mountains of the Moon, is founded on the statements of the celebrated astronomer and geographer Claudius Ptolemæus of Alexandria, who flourished in the second century of the Christian æra. One thing is certain, which is, that he is the *first* writer by whom the Mountains of the Moon are mentioned; and it is most probable that he derived his information respecting them from the Greek traders of Alexandria, who, from the time of Hippalus's discovery of the monsoons in the middle of the preceding century, if not previously, frequented the east coast of Africa.

Our only sure method of proceeding seems, then, to be, that we should refer back to the *original* statements of Ptolemy; and that, without regarding the numerous commentaries

* *Reise zur Entdeckung der Quellen des Weissen Nil.* 8vo. Berlin, 1848.

† See Edinburgh New Philosophical Journal, vol. xlv. p. 221 *et seq.*

and glosses upon those statements, we should ourselves examine and attempt to elucidate them with the help of the extended geographical knowledge which we possess at the present day.

Now, in the 9th chapter of the 4th book of his Geography, Ptolemy describes the eastern coast of Africa as stretching "towards the east from Cape Rhaptum on the Barbarian Gulf, which is also called the Rough Sea on account of the shoals, as far as Cape Prasum; beyond which the country is unknown." And he proceeds to describe Cape Prasum as being situate in 80° longitude east of Ferro, and 15° S. lat.; and that "near it, towards the north-east, is an island, named Menuthius, which lies in 85° E. long. and $12^{\circ} 30'$ S. lat." He adds, that "round the gulf dwell certain cannibal negroes (*Æthiopes Anthropophagi*), on the west of whose country are the Mountains (or hill-country) of the Moon—*τὸ τῆς Σελήνης ὄρος*—the snows of which are received into the lakes of the Nile." And he describes these Mountains of the Moon as lying in $12^{\circ} 30'$ S. lat.; the one extremity of them being in 57° , and the other in 67° E. long.

On testing, by means of the information possessed by us at the present day, the results arrived at by the geographer of Alexandria, we are at once struck with the great extension in a southerly direction, which Ptolemy has given to the courses of the Nile and its two great tributary streams, the Astaboras and Astapus, as likewise to the eastern coast of Africa, as far as it was then known. For the correction of this fundamental error our means are two. The one is the positive knowledge respecting the courses of the rivers themselves, which has been acquired from the recent Egyptian expeditions up the Nile and the explorations of travellers in Abessinia; the other is the like positive information derived from the surveys made of the east coast of Africa, and from the particulars respecting the interior of the Continent collected at various points along the coast.

From the former of these sources of information we are enabled to lay down, with almost absolute accuracy, the course of the Astaboras, now known as the Atbara or *Tákkazie*, and that of the Astapus, Blue River or *Abái*. From the same source we further learn that in about 9° N. lat. the main stream of the Nile divides into three arms; namely—1st, the *Bahr el Abyad* or White River, which has been ascended to about the 4th parallel of N. lat.; 2nd, the *Sobát*, *Telfi* or River of Habesh, which falls into the central stream from the east, and is considered to contribute to the Nile nearly a moiety of its waters; and 3rd, the *Bahr el Ghazál* or *Keíláh*,

which joins the Nile from the west, and is described as being a magnificent stream, with a tolerably rapid current.

It will be observed, however, that by Ptolemy the main stream of the Nile is laid down as consisting of *two* principal arms, the junction of which is placed by him in the second parallel of north latitude, or nearly seven degrees to the south of where the junction of the *three* principal arms actually takes place. In order to prevent misunderstanding, it is proper to direct attention to the fact, that I am not alluding here to the confluence of the Baħr el Abyaḍ and Baħr el Azreḱ—the White and Blue *Rivers*—which are commonly but erroneously called the White and Blue *Niles*. This confluence takes place at Khartúm in $15^{\circ} 37'$ N. lat.; and, as it will be plainly seen, it is merely the junction of the Astapus with the Nilus. Ptolemy's bifurcation of the Nile is, on the contrary, formed by the union of the Sobát, or River of Habesh, with the White River, in $9^{\circ} 20'$ N. lat., more than six degrees of latitude to the south of Khartúm. It is important to bear this in mind; as one of the most fertile sources of error in the consideration of this subject, has been the regarding of the Blue River, or "Bruce's Nile" as it is frequently called, as one of the main arms of Ptolemy's Nile, which it manifestly is not.

Turning now to the east coast of Africa, our first point is to identify the island of Menuthias; and though the subject is not altogether free from difficulties, the greatest amount of probability is certainly in favour of the island of Zanzibar. As it is essential to the investigation of the subject that we should advance from some fixed point, it shall be assumed that the identity of Zanzibar with Menuthias is established. And such being the case, it follows that the Barbarian Gulf is the bay or bight in which Zanzibar is situate; and that the country of the Anthropophagi or cannibals dwelling round this gulf, is that portion of the east coast of Africa which is opposite to that island. Further, as the Mountains of the Moon are stated by Ptolemy to lie on the western side of this country of the Anthropophagi, *and in the same latitude as Menuthias*, we can have no hesitation in placing those mountains somewhere in that part of the main land which, in like manner, lies opposite to Zanzibar.

If, now, we apply to these general results the knowledge which we possess of the physical configuration of the plateau of Eastern Africa, of which a brief outline has already been given, we shall perceive that the seaward or eastern edge of this plateau, when viewed from the lowlands along the shores of the Indian Ocean, presents the appearance and indeed possesses the character of an extensive range of lofty moun-

tains. And that portion of this range which bounds the country of Mono-Moézi, and which lies in a general direction to the west or north-west of the island of Zanzibar, corresponds so satisfactorily with the "Mountains of the Moon," described by Ptolemy as lying on the western side of the country of the Anthropophagi who dwelt on the shores of the Barbarian Gulf near Menuthias, that we may, with every show of reason, consider the identification as absolute, and place here the source of Ptolemy's eastern arm of the Nile.

The name *Mono-Moézi* itself affords a strong argument in corroboration of this conclusion. This expression is a compound word, significant in many of the languages of the Kafir class, which are spoken throughout the entire continent of Africa south of the Equator, as far as the limits of the Hottentots. The first component of this name, *Mono* or *Mani*, is of frequent occurrence in the designations of countries in Southern Africa, such as Mani-Congo, Mani-Puto (as the Portuguese possessions in Africa are called), Mono-Motapa, &c.; and its meaning appears to be *king* or *ruler*. The second component, *Moézi*, which alone is properly the name of the country, has the signification of *moon* in the languages and dialects of the Sawáhilis and of the natives of the countries of Mono-Moézi, Congo, Mozambique, and various others. The Sawáhilis, among whom the word is thus significant, are the inhabitants of the sea-coast of Zindj, or Zangebar; and I conceive that the Greeks of Alexandria who traded with that coast, obtained from these Sawáhilis the particulars respecting the eastern portion of the African continent and the sources of the Nile, which are recorded by their countryman the geographer Ptolemy; and that, as it was not an unusual practice among the Greeks to *translate* significant proper names into the equivalents in their own language, the designation given by Ptolemy to the mountains in which those sources are situated—*Σελήνης ὄρος*, "the mountains, or hill country, of the moon"—is simply a translation of the Sawáhili expression, "the Mountains of Moézi*."

* The discovery of Mount Kilimandjáro, covered with perpetual snow, recently announced by the Rev. J. Rebmann (see Church Missionary Intelligencer for May 1849, vol. i. p. 17 *et seq.*), affords an additional argument in support of the above hypothesis. Ptolemy states that "the lakes of the Nile receive the *snows* of the Mountains of the Moon;" the upper course of the direct stream of the river has been carried southwards to about 2° S. lat. and 34° E. long.; and Mount Kilimandjáro, which is crossed by the road from the coast to the country of Mono-Moézi, is placed by Mr. Rebmann in 3° 40' S. lat. and 36° E. long. Hence the snow-capped Kilimandjáro may be regarded as forming part of the "Mountains of the Moon (Moézi)," in which the Nile has its origin.

Mr. Rebmann mentions that the natives have no specific name for *snow*,

It is unnecessary to go into further details to show the reasonableness of the opinion that these "Mountains of the Moon" form the south-eastern rather than the southern limits of the basin of the Nile. As regards the southern limits themselves, no more can be said, in the existing state of our knowledge (or want of knowledge) on the subject, than that they are most probably formed on the east by the basin of the Lufídjí or some other large river flowing into the Indian Ocean, and on the west by that of the Upper Congo. It must however not be omitted to be mentioned, that we possess evidence of the existence, somewhere in this direction, of a great lake, which is very incorrectly shown in the ordinary maps under the title of Lake Maravi, and which is more correctly designated *Nyassi* or "the sea;" an expression apparently equivalent to the Arabic *Bahr*, which is used indefinitely to express a sea, a lake, or a large river,—the "great water," in fact, of Africans generally; but whether this "*Nyassi*" is connected with the upper course of *the Nile*, or of the Congo, or of the Lufídjí, or whether it possesses a separate hydrographical system of its own, like the Caspian Sea, we have not for the present the means of determining. Should the latter happen to be the case, then it may be the basin of this inland lake, and not those of rivers flowing in opposite directions into the Atlantic and Pacific Oceans, which forms the southern boundary of the river system of the Nile. But however the case may eventually be found to be, the result will not materially affect the arguments already used on the subject; and, at all events, some of the north-eastern arms of the Congo will still have their sources at the waterparting between them and the extreme south-western branches of the Nile. It is to be hoped that Dr. Bialloblotzky, who is on his way to explore those regions, will be enabled to clear up some of these dark questions of African geography*.

It only remains for us to consider the western limits of the basin of the Nile; upon which subject an almost total want of positive information precludes us from saying much.

According to the information furnished to M. Werne by Lakono, king of Bari, the direct stream of the Bahr el Abyad, there called Tubiri, comes from a distance of thirty days'

which they call *coldness*. But in this there is nothing singular. In the Arabic and Amharic languages, *běřed* means *cold* (coldness) generally, while *barad* (in Amharic more frequently *barado*)—which is substantially the same—means *hail* specifically.—July 11, 1849.

* Since this paper was read this traveller has been compelled to abandon his undertaking. It must now be left to more favoured explorers to perform what was hoped for from him.

journey to the south. But M. Lafargue, a French traveller who ascended the Bahr el Abyad in 1845, states that he was informed that the main stream comes from the *west* at a distance of only six days' journey south of Bari. It will be seen that this information is not necessarily opposed to that of M. Werne, since both arms may well exist; but it shows also that we are still in ignorance with respect to an extensive and probably an immense division of the basin of the Nile—namely, the whole of that portion of it which lies to the west of the direct stream ascended by the Egyptian expeditions. In the absence of any certain data, it would be mere speculation to attempt to fix the waterparting in this direction, except that we know that it cannot possibly lie much, if anything, beyond the 20th meridian of east longitude, where the basin of the Nile must necessarily be limited by that of Lake Tchad.

We will therefore descend the main stream of the White River northwards as far as $7^{\circ} 43'$ N. lat., where the officers of the second Egyptian expedition found it to receive three branches from the south and west. These tributaries were considered to be of no importance, and to *appear* to proceed only from the neighbouring marshes. But they require to be more closely examined before it can positively be asserted that they are not streams of some, perhaps even considerable importance. Thence continuing to descend the river, we come at length, in $9^{\circ} 20'$ N. lat., to the great western arm, called Bahr el Ghazál or Keiláh, which has been already described. The first exploring party was almost in doubt whether this was not the principal arm of the river; but as their instructions from the Pasha were imperative as to their exploring the *direct* stream, they did not afford it that attention which it evidently deserves. M. Lafargue, who subsequently entered the mouth of this river, describes it as a magnificent stream with a tolerably rapid current.

M. Werne, in speaking of the Bahr el Ghazál, states that it was described to him as coming *from Barbary*! This our positive knowledge enables us to assert to be impossible. For Denham and Clapperton crossed the continent of Africa from the Mediterranean to Lake Tchad, in about the 15th meridian of east longitude; and we know that there does not exist on their route any watercourse which might be considered as the upper portion of this river. It would, however, doubtless be wrong to understand the expression “Barbary” in the sense in which we usually employ it; as it is not to be imagined that the inhabitants of the valleys of the Bahr el Abyad and Bahr el Ghazál have any actual knowledge of that portion of Africa to which we give this name. We must therefore suppose that,

in speaking of the origin of the latter river, they made use of some expression equivalent to the Arabic *Belâd-el Gharb*; which, though the name given by Orientals to Barbary, means literally *the west country*, Barbary being so called simply because it lies to the west of them. Under this view of the case, the statement must be understood to have been, not that this western arm of the Nile rises in Barbary, but merely that it comes from some country *in the west**.

The fact of the existence of this great western arm, which is beyond all question, gives rise to serious considerations. More especially it requires us to re-investigate a statement made by the historian Herodotus respecting the upper course of the Nile—a statement which geographers of modern times have concluded could only be applicable to the Niger, Joliba or Kwara (Quorra) of Western Africa.

In treating, in his second book, of the origin of the Nile, the historian says:—"This river flows from the west and the setting of the sun; but beyond this no one is able to speak with certainty, for the rest of the country is desert by reason of the excessive heat. But I have heard the following account from certain Cyrenæans, who say that they went to the oracle of Ammon, and had a conversation with Etearchus king of the Ammonians; and that, among other subjects, they happened to discourse about the Nile,—that nobody knew its sources: whereupon Etearchus said, that certain Nasamonians once came to him; this nation is Libyan and inhabits the Syrtis, and the country for no great distance eastward of the Syrtis; and that when these Nasamonians arrived, and were asked if they could give any further information touching the deserts of Libya, they answered, that there were some daring youths amongst them, sons of powerful men; and that they, having reached man's estate, formed many other extravagant plans, and moreover chose five of their number by lot to explore the deserts of Libya, to see if they could make any further discovery than those who had penetrated the furthest. . . . That when the young men deputed by their companions set out, well-furnished with water and provisions, they passed first through the inhabited country; and having traversed this, they came to the region infested by wild beasts; and after this they crossed the desert, *making their way towards the west*; and when they had traversed much sandy ground, during a journey of many days, they at length saw some trees growing in a plain; and that they approached and began to gather the fruit that grew on the trees; and while they were gathering, some diminutive men,

* *Algarve*, the southernmost province of the kingdom of Portugal opposite Barbary, derives its name from the Arabic *el-Gharb*.

less than men of middle stature, came up, and having seized them carried them away; and that the Nasamonians did not at all understand their language, nor those who carried them off the language of the Nasamonians. However, they conducted them through vast morasses, and when they had passed these, they came to a city, in which all the inhabitants were of the same size as their conductors, and black in colour: and by the city flowed a great river, *running from the west to the east*, and that crocodiles were seen in it. Thus far I have set forth the account of Etearchus the Ammonian; to which may be added, as the Cyrenæans assured me, that he said the Nasamonians all returned safe to their own country, and that the men whom they came to were all necromancers. Etearchus also conjectured that this river, which flows by their city, is the Nile; and reason so evinces: for the Nile flows from Libya, and intersects it in the middle, and (as I conjecture, inferring things unknown from things known) it sets out from a point corresponding with the Ister. . . . So I think that the Nile, traversing the whole of Libya, may be properly compared with the Ister. Such, then, is the account that I am able to give respecting the Nile*."

The objection usually made to the supposition that this river of Herodotus is the upper course of the Nile, is, that the Nasamonians travelled, not southwards but westwards, and that consequently they could not have reached any river but the Niger. But, if the historian's expressions are to be taken as strictly meaning that the road lay *throughout* in a westerly direction, it is manifest that it would be just as impossible for the travellers to have arrived at the Niger as to have reached a western arm of the river of Egypt. In either case, then, we are bound to suppose that they first proceeded *southwards*—to what distance we cannot say—and then, by continuing during the last part of their journey in a westerly direction (which is in no wise to be understood as meaning *due west*), they may without difficulty have come to the Nile, that is to say, to the upper course of this great western arm, possibly at no very great distance to the east or north-east of Lake Tchad.

The mention of this lake induces me to suggest the probability that Nyassi and Tchad, the one at the extremity of the southernmost and the other at the extremity of the westernmost arm of the Nile, are the originals of the two lakes which Ptolemy describes as receiving the snows of the Mountains of the Moon.

It has merely to be added, that, according to a sketch-map

* Lib. ii. § 31-34; Cary's translation.

of Darfúr, drawn at Cairo in 1841 by Dr. Perron, under the dictation of Sheikh Mohammed el Túnisi, there is a large river in the west of that country named Bare; and that from the evidence collected by M. Jomard in his Preface to the *Voyage au Darfour* of the Sheikh, it may be concluded that both that country and Kordofán lie within the basin of the Nile, and that this river Bare is an affluent of the Bahr el Ghazál of the Egyptian expeditions.

We have thus brought the limits of the hydrographical system of the Nile round again to the confines of Nubia and Egypt; in which countries, as it is well known, the basin of that river, on the western as on the eastern side, consists of little more than the bed of the stream itself.

St. Mildred's Court,
Dec. 30, 1848.

XV. *On the Effect of surrounding Media on Voltaic Ignition.*
By W. R. GROVE, Esq., M.A., V.P.R.S.*

IN the Philosophical Magazine for December 1845, I pointed out a striking difference between the heat generated in a platinum wire by a voltaic current, according as the wire is immersed in atmospheric air or in hydrogen gas, and in the Bakerian Lecture for 1847 I have given some further experiments on this subject, in which the wire was ignited in atmospheres of various gases, while a voltameter enclosed in the circuit yielded an amount of gas in some inverse ratio to the heat developed in the wire. It was also shown, by a thermometer placed at a given distance, that the radiated heat was in a direct ratio with the visible heat.

Although the phænomenon was apparently abnormal, there were many known physical agencies by which it might possibly be explained, such as the different specific heats of the surrounding media, their different conducting powers for electricity, or the varying fluency or mobility of their particles which would carry off the heat by molecular currents with different degrees of rapidity.

The investigation of these questions will form the subject of this paper.

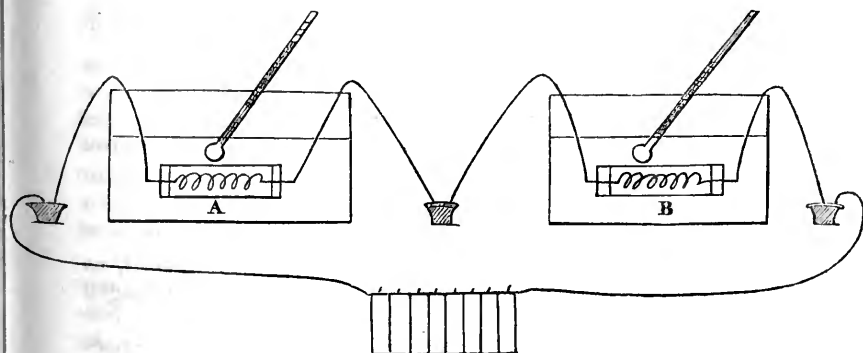
An apparatus was arranged, see fig. 1. Two glass tubes A and B, of 0·3 inch internal diameter and 1·5 inch length, were closed with corks at each extremity; through the corks the ends of copper wires penetrated, and joining these were coils of fine

* From the Philosophical Transactions for 1849, part i.; having been received by the Royal Society August 10, and read December 14, 1848.

platinum wire, one-eightieth of an inch diameter and 3·7 inches long when uncoiled. Tube A was filled with oxygen, tube B with hydrogen, and the tubes thus prepared were immersed in two separate vessels, in all respects similar to each other, and containing each 3 oz. of water. A thermometer was placed in the water in each vessel; the copper wires were connected, so as to form a continued circuit, with a nitric acid battery of eight cells, each plate exposing eight square inches of surface. Upon the circuit being completed the wire in the tube containing oxygen rose to a white heat, while that in the hydrogen was not visibly ignited; the temperature of the water, which at the commencement of the experiment was 60° F. in each vessel, rose in five minutes in the water surrounding the tube of hydrogen from 60° to 70°, and in that containing oxygen from 60° to 81°*.

Before I enter into a further detail of experiments, I would remark upon the extraordinary character of this result. The same current or quantity of electricity passes through two similar portions of wire immersed in the same quantity of

Fig. 1.



* After the publication of the Bakerian Lecture, my experiment on the peculiar effect of hydrogen on the ignited wire was noticed in a paper by M. Matteucci, which though I had it in my hand shortly after its publication, I regret to say I did not read with the attention it deserved. I have read it since the experiments in this paper were commenced, and I see that I am now executing a task assigned to me by my friend. M. Matteucci, for a different object, makes a somewhat similar experiment to the one given above, which however differs from mine in the material point, that he operated first on one gas and then on the other, and thus did not compare the effects produced by the same quantity of electricity. I cannot quite agree in the conclusions deduced by him from this and the other experiments he cites, but I will not here contest them, as it would lead me away from the main point of this paper.

liquid, and yet, in consequence of their being surrounded by a thin envelope of different gases, a large portion of the heat which is developed in the one portion appears to have been annihilated in the other. Similar experiments, varying the gas in one tube while hydrogen was retained in the other, gave the following results. In five minutes the thermometer rose—

1st.	In the hydrogen. From 60° to 69°·5.	In the associated nitrogen. From 60° to 81°·5.
2nd.	In hydrogen. From 60° to 70°·5.	In carbonic acid. From 60° to 80°.
3rd.	In hydrogen. From 60° to 70°.	In carbonic oxide. From 60° to 79°·5.
4th.	In hydrogen. From 60° to 70°·5.	In olefiant gas. From 60° to 76°·5*.

On a different day I tried the following experiments; all the circumstances were the same, excepting that the battery was in more energetic action, for which reason I have not tabulated them with the others.

In oxygen associated with coal gas the thermometer rose in five minutes—

In oxygen. From 60° to 82°.	In coal gas. From 60° to 76°.
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In hydrogen associated with coal gas the thermometer rose in five minutes—

In hydrogen. From 60° to 77°.	In coal gas. From 60° to 82°·5.
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From this it would appear that coal gas should be placed, as to its cooling effect on the ignited wire, between hydrogen and olefiant gas.

On another day sulphuretted hydrogen associated respectively with oxygen and hydrogen was tried; the wire in the sulphuretted hydrogen was at first ignited to a degree somewhat inferior to that in oxygen, but the gas was rapidly decomposed; sulphur being deposited on the interior of the vessel and the intensity of ignition gradually decreased, so as ultimately to be scarcely superior to the ignition in hydrogen:

* I should perhaps remark, that several test experiments were tried to ascertain the working of the apparatus; thus, the same gas was placed in both tubes, and the results given by the thermometer were found to be accurately the same in both vessels. The tubes were also changed with reference to the containing vessels and to the contained gases. The water was always agitated to render its temperature uniform previously to reading off, &c. &c.

indeed the gas by this time had become nearly pure hydrogen. The following were the effects on the thermometer in five minutes, all being arranged as before:—

In oxygen.
From 60° to 86°.

In sulphuretted hydrogen.
From 60° to 76°.

In hydrogen.
From 60° to 79°.

In sulphuretted hydrogen.
From 60° to 81°·5.

This result would place sulphuretted hydrogen between hydrogen and coal gas; but as the gas was rapidly decomposed, the greater part of the experiment was made with hydrogen containing small quantities of sulphur combined, and not with sulphuretted hydrogen. I therefore think that proto-sulphuret of hydrogen, or the gas which consists of equivalent ratios of the two elements, would be much further removed from pure hydrogen; probably it would be about equal in its cooling effect to carbonic acid or carbonic oxide.

In phosphuretted hydrogen the platinum wire is destroyed by combining with the phosphorus the instant it reaches ignition, so that its relation to the other gases could not be ascertained.

Protoxide and deutoxide of nitrogen are, as I have observed in the Bakerian Lecture, decomposed by the ignited wire; they, as well as atmospheric air, are, as nearly as may be, equal in their effect to their elements separately.

In the vapour of æther the ignited wire is extinguished nearly as completely as in hydrogen; I have not yet tried its comparative effect, but should judge it to be nearly the same as coal gas or olefiant gas.

In my former experiments* the following was the order of the gases, testing the intensity of ignition by the inverse conducting power of the wire, as measured by the amount of gas in a voltameter included in the circuit.

Gases surrounding the wire.	Cubic inches of gas evolved in the voltameter per minute.
Hydrogen	7·7
Olefiant gas	7·0
Carbonic oxide	6·6
Carbonic acid	6·6
Oxygen	6·5
Nitrogen	6·4

Assuming that in the present experiments the heat in the water is a correct indication of the intensity of ignition in the wire, the order is the same in both series of experiments.

* Philosophical Transactions, 1847, p. 2. [Phil. Mag. S. 3. vol. xxxi. p. 21.]

Hydrogen is however so far removed from both oxygen and nitrogen in its effects upon the ignited wire, that in order more accurately to ascertain the relative position of the latter two gases, I made a few further experiments on them as contrasted with each other, and not with hydrogen. I first repeated my former experiment on these two gases, varying it only by changing the circumstances in the manner suggested by the present experiments, which on account of the vessel containing the wire being immersed in a given quantity of water, instead of being exposed to the external atmosphere, would occasion greater equality in the surrounding cooling effects, and would give me the opportunity of combining both methods in one experiment.

I filled both tubes A and B with oxygen, and included a voltameter in the circuit; in two minutes 3·43 cubic inches of hydrogen were evolved in the voltameter, and the thermometer in each cell had risen from 60° to 63°. A similar experiment with nitrogen gave in two minutes 3·4 cubic inches of hydrogen, and the thermometer rose from 60° to 63°.

This experiment accords with my previous one as to the voltameter test, but indicates no difference in oxygen and nitrogen with the thermometer test; I therefore in the following three experiments associated nitrogen with oxygen in the apparatus, fig. 1. All things being disposed as with the experiments on hydrogen associated with other gases, in five minutes the thermometer rose—

	In the oxygen.	In the associated nitrogen.
Exp. 1st.	From 60° to 71°·5.	From 60° to 73°.
2nd.	60° to 77°.	60° to 76°.
3rd.	60° to 75°.	60° to 76°.
Mean . . .	60° to 74°·5.	60° to 75°.

The battery had increased somewhat in power after the first experiment, but as both wires formed part of the same circuit in each experiment, the variations in battery power do not affect the comparative results. The second experiment gives a variation in the position of oxygen and nitrogen with reference to the first and third experiments, but the gases so nearly approach in their cooling effects, that these slight differences are not much to be relied upon; however I applied a further test. I associated in turn oxygen and nitrogen with carbonic acid; the following were the results. In five minutes the thermometer rose—

	In oxygen.	In carbonic acid.
Exp. 1st.	From 60° to 75°.	From 60° to 75°.
2nd.	60° to 76°.	60° to 75°.

	In nitrogen.	In carbonic acid.
Exp. 1st.	From 60° to 74°.	60° to 73°.
2nd.	60° to 73°.	60° to 72°·5.

The battery had in the last experiment a little decreased in power; the oxygen and nitrogen both produced a less cooling effect than the carbonic acid, but the oxygen came nearer to it than the nitrogen, thus according with the previous experiments. Upon the whole it would appear that oxygen produces a somewhat greater cooling effect on the ignited wire than nitrogen, but these gases may, for the purposes of this paper, be fairly regarded as equal. Atmospheric air produces a similar effect to oxygen and nitrogen separately, though I am inclined to think that a slight chemical change takes place when atmospheric air is exposed to the ignited wire, and that nitrous acid is formed; for if litmus paper be held over a voltaically ignited platinum wire in the air, a slight but very perceptible tinge of red marks the portion of it immediately over the wire.

With the view of ascertaining whether the specific heat of the surrounding media were the cause of the phænomenon, I proceeded to try the effect of the wire carrying a voltaic current on different liquids; all things being disposed as in the previous experiments, and 3 oz. of water being associated respectively with the same quantity of the following liquids. The thermometer rose in five minutes—

In water, from 60° to 70°·3. In spirit of turpentine 60° to 88°.

In water, from 60° to 70°·3. In sulphuret of carbon 60° to 87°·1.

In water, from 60° to 69°. In olive oil . . . 60° to 85°.

In water, from 60° to 70°·1. In naphtha . . . 60° to 78°·8.

In water, from 60° to 70°·5. In alcohol sp. gr. 0·84 60° to 77°.

In water, from 60° to 68°·5. In æther . . . 60° to 76°·1.

I do not much rely on the last experiment,—the battery was in more feeble action; and though each of the above results is the mean of three experiments, yet the variations in the results of the different experiments with æther being considerable (while in the others they were very trifling), lead me to place no great dependence on it. The rapidity of evaporation and the readiness of ebullition of the æther require that a larger quantity should be used; but as this for the purpose of comparison would have required all the experiments to be repeated with different quantities of liquid, I have not thought it worth while to go through the series a second time. It will

be observed, that the effects with the above liquids are by no means in direct relation with their respective specific heats; but in order to bring the results of the experiments with liquids into comparison with those with gases, I now associated a gas with a liquid, viz. hydrogen with water. All things being disposed as before, the tube A was filled with hydrogen gas, the tube B with water, both being immersed in 3 oz. of water. The thermometer rose in five minutes—

In hydrogen.
From 60° to $75^{\circ} \cdot 5$.

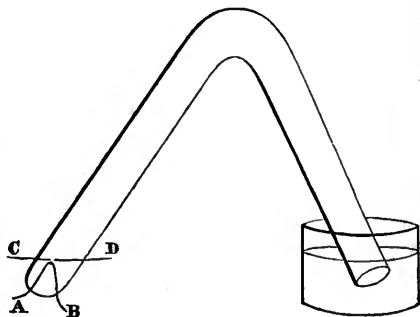
In water.
From 60° to 72° .

This experiment of itself conclusively negatives the possibility of specific heat alone accounting for the phenomenon under consideration; and though, doubtless, specific heat must have some influence on the cooling effects of different gases and liquids, yet in the former it is apparently of very trifling import in comparison with the real physical cause of the differences, whatever that may be.

Supposing, as is stated by Faraday*, that gases possess feeble conducting powers for voltaic electricity, and supposing hydrogen, from its close analogy in chemical character to the metals, to possess a greater conducting power than the other gases, this would account for its peculiar effect on the ignited wire, as a certain portion of the current, instead of forcing its way through the wire, would be carried off by the surrounding gas. In order to ascertain this I arranged the following experiments.

1st. Into the closed end of a bent tube, fig. 2, a loop of platinum wire, AB, and two separate platinum wires CD, were hermetically sealed, the extremities of the latter being approximated as closely as possible, and the interval between them being close to and immediately over the apex of the loop. The tube was filled with hydrogen, and the wire AB connected with a voltaic battery of sufficient power to raise it to as high a degree of ignition as it would bear without fusion; C and D were now connected

Fig. 2.



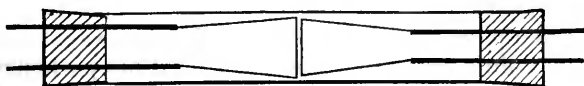
* Experimental Researches, §§ 272, 441 and 444

with the poles of another battery, a delicate galvanometer being interposed in the circuit. Not the slightest effect on the galvanometer needle could be detected, and a similar negative effect took place when the tube was filled with atmospheric air.

2nd. Parallel portions of platinum wire were now arranged in close proximity (see fig. 3.), and so that each might be ignited to a full incandescence by separate insulated batteries. When surrounded by atmospheres, both of atmospheric air and of hydrogen, and fully ignited, not the slightest conduction could be detected, across the interval between the wires, with ten cells of the nitric acid battery, and being enabled by the kindness of Mr. Gassiot to repeat this experiment with his battery of five hundred well-insulated cells of the nitric acid combination, air did not conduct when the ignited wires were approximated to the one-fiftieth of an inch; on approaching them nearer they came within striking distance, were instantly fused, and the galvanometer needle, which had up to this time been perfectly stationary, was whirled rapidly round.

I think I am entitled to conclude from this, that we have no experimental evidence that matter in the gaseous state con-

Fig. 3.



ducts voltaic electricity; probably gases do not conduct Franklinic electricity, as the experiments which would seem *prima facie* to lead to that conclusion, are explicable as resulting from the disruptive discharge.

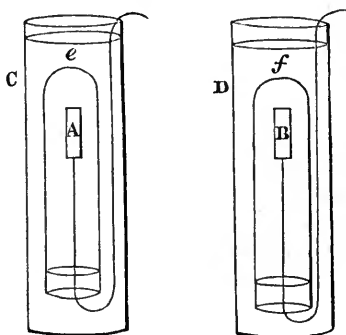
In Faraday's experiment two wires were approximated in the flame of a spirit-lamp, and a slight conduction across the interval in the flame was observed. This conduction might have been due to certain unconsumed particles of carbon existing in the flame, or possibly to the flame itself; according to Dr. Andrews, flame, even that of pure hydrogen gas, conducts voltaic electricity*.

I now endeavoured to ascertain whether any specific inductive effect of the hydrogen might have an influence: parallel wires of platinum and parallel coiled copper wires were placed in atmospheres of hydrogen and of atmospheric air, one of which parallel wires conveyed the current, and the other wire was connected with a delicate galvanometer. I could detect no

* Phil. Mag., vol. ix. p. 176.

difference in the arcs of deflection of the needle at the instant of meeting or breaking contact, whether the wires were in atmospheres of hydrogen or of atmospheric air; nor when parallel platinum wires with their surrounding atmospheres of gas were immersed in a given quantity of water, could I detect any difference in the resulting heat, whether the current passed in the same or in a different direction through each wire.

My next object was to ascertain whether, in cases of ordinary ignition, the same apparent annihilation of heat took place in hydrogen gas as with voltaic ignition. Two iron cylinders AB, fig. 4, each weighing 390 grains, were attached to long iron wires bent back in the form shown in the figure. The cylinders were placed together in a crucible of fine sand, and the whole heated to a uniform white heat. The cylinders were now taken out of the sand, placed at the surface of equal portions of water in the vessels C and D; two inverted tubes, *e*, *f*, the one of hydrogen, the other of atmospheric air, were placed over them, and the whole quickly immersed in the water, and retained by a little contrivance, which I need not particularize, in the position shown in the figure. The temperature of the water at the commencement of the experiment was 60° Fahr.



In four minutes the water surrounding the hydrogen had risen to 94°, and became stationary there, while that surrounding the air had only reached 87°; in ten minutes the water surrounding the hydrogen had sunk to 92°·5, while that surrounding the air had risen to 93°, which was the highest temperature it reached; thus the respective maxima were 94° and 93°; but considering the greater time which the water surrounding the air required to attain its maximum temperature, and that being during this time at a temperature above that of the surrounding atmosphere, it must have lost something of its acquired heat, we may fairly consider the maxima to be the same, and that the difference of effect in the two gases had reference solely to the time occupied in the transference of the heat. In a second experiment the results were similar, the maximum being in this experiment 92·5 in hydrogen, and 91 in air*.

* Iron wire produces a similar effect to platinum wire in the voltaic experiments.

As far as ordinary ignition is concerned, hydrogen has been shown by the experiments of Leslie and Davy to produce a more rapid cooling effect than air; and the above experiment having shown that it does not alter or convert into any other force the actual amount of heat given off, my next step was to inquire whether this rapidity of cooling effect of the hydrogen would account for the effects observed with voltaic ignition. Although the two classes of effects were apparently very different, it might be that the improved power of conduction arising from the rapid cooling effect of the hydrogen might, by enabling the current to pass more readily, carry off the force in the form of electricity, which if the wire offered more resistance (as it would when more highly ignited) would be developed in the form of heat. By employing the same medium, but impeding the circulation of the heated currents in one case, while their circulation was free in the other, some light might be expected to be thrown on the inverse relation of the conducting power to the heat developed. The following experiment was therefore tried.

In the apparatus represented in fig. 1, tube A was uncorked, so as to allow free passage for the water, while tube B was filled up with fine sand soaked with water, and then corked at both ends; the current was passed and the following was the result. In the vessel containing tube A, the thermometer rose in five minutes from 52° to 60° , and in that containing tube B from 52° to 60° also; during a second five minutes, the thermometer rose in the vessel containing A from 60° to 67° , and in the vessel containing B from 60° to 67° also.

I tried another analogous experiment: a coil of platinum wire was placed in a very narrow glass tube one-sixth of an inch diameter; this was hermetically sealed at one end, and the other drawn into a very narrow aperture, little more than sufficient to allow the platinum wire to pass, and filled with water (it was necessary to leave a small aperture to prevent the bursting of the tube by the expansion of the heated water); in the other vessel a similar coil of platinum wire was placed, but without any glass tube at all. The circuit having been completed as before, the thermometer rose in five minutes—

In the water without the tube, from 60° to 87° .

In the water containing the tube, from 60° to 86° .

Here the difference, slight as it was, was against what theory would have led one to anticipate; the exact equality however of the previous experiment, and the close approximation of the results in this one, afford no conclusive information as to the point under consideration, though the negative result

rather tends against the view which would assimilate the effects of voltaic to those of ordinary ignition.

As another method of attaining the object before mentioned, viz. the inverse relation of the conducting power of the wire to the heat developed in it, I tried the following experiment. A platinum wire of one foot long and one-eightieth of an inch diameter was ignited in air by ten cells of the battery, a voltmeter being included in the circuit; the amount of hydrogen given off by the voltmeter was one cubic inch in forty-four seconds: half the wire was now immersed in water of the temperature of 60° F.; by this means the intensity of ignition of the other half was notably increased; the voltmeter now yielded one cubic inch in forty seconds: two-thirds of the wire immersed, gave one cubic inch in thirty-seven seconds; and five-sixths immersed, gave one cubic inch in thirty-five seconds. The heat of the portion of wire not immersed in water had in the last experiment nearly reached the point of fusion of the platinum. By this result it appears that the increased resistance to conduction of the ignited portion is not equal to the increased conducting power of the cooled portion of the same wire.

With a view of seeing how far the cooling effect upon the ignited wire might be due to the greater or less fluency or mobility of the particles of the different media surrounding it, I have looked into the papers of Faraday* and of Graham†. In the experiments of the former, it appears that the escape of different gases at a certain pressure through capillary tubes, or the velocities of revolution of vanes or floats surrounded by different gases, was in some inverse ratio to the density of such gases; and the experiments of the latter show that the effusion or escape of gases through a minute aperture in a plate, takes place with velocities inversely as the square root of their specific gravities. In Graham's experiments, however, when the escape took place through capillary tubes, the results seemed subject to no ascertained law, though the compounds of carbon with hydrogen passed through with greater facility than other gases.

The cooling effects of gases on the ignited wire are decidedly not in any ratio with their specific gravities; thus, carbonic acid on the one hand, and hydrogen on the other, produce greater cooling effects than atmospheric air; and olefiant gas, which closely approximates air, and is far removed from hydrogen in specific gravity, much more nearly approximates hydrogen, and is far removed from air in its cooling effect.

* Quarterly Journal of Science, vol. iii. p. 354.

† Philosophical Transactions, 1846, p. 573.

Upon the whole, we may conclude, from the experiments detailed in this paper, that the cooling effect of different gases, or rather the difference in the cooling effect of hydrogen and its compounds from that of other gases, is not due to differences of specific heat; it is not due to differences of specific gravity; it is not due to differences of conducting powers for electricity; it is not due to the character of hydrogen in relation to its transmission of sound, noticed by Leslie, for reasons which I have before given*; it is not due to the same physical characters of mobility which occasion one gas to escape from a small aperture with greater facility than another; but it may be, and probably is, affected by the mobile or vibratory character of the particles by which heat is more rapidly abstracted. I at one time thought that the effect might have relation to the combustible character of the gas, and that the electro-negative gases were in respect to it contra-distinguished from the electro-positive or neutral gases, but the experience I have obtained from the experiments detailed here induces me to abandon that supposition.

I incline to think, that, although influenced by the fluency of the gas, the phænomenon is mainly due to a molecular action at the surfaces of the ignited body and of the gas. We know that in the recognised effects of radiant heat, the physical state of the surface of the radiating or absorbing body exercises a most important influence on the relative velocities of radiation or absorption; thus, black and white surfaces are, as every one knows, strikingly contra-distinguished in this respect: why may not the surface of the gaseous medium contiguous to the radiating substance exercise a reciprocal influence? why may not the surface of hydrogen be as black, and that of nitrogen as white to the ignited wire? This notion seems to me the more worthy of consideration as it may establish a link of continuity between the cooling effects of different gaseous media and the mysterious effects of surface in catalytic combinations and decompositions by solids such as platinum. Epipolic actions will, I feel convinced, gradually assume a much more important place in physics than they have hitherto done; and the further development of them appears to me the most probable guide to the connexion by definite conceptions of physical and chemical actions.

The difference of the cooling effect of hydrogen, and of those of its compounds, where it is not neutralized by a powerful electro-negative gas, from all other gases, is perhaps the most striking peculiarity of the phænomena I have described.

* Philosophical Transactions, 1847. [Phil. Mag. S. 3. vol. xxxi. p. 22.]

The differences of effect of all gases other than hydrogen and such compounds are quite insignificant when compared with the differences between the hydrogenous and the other gases. There are some phænomena which I have before observed, and which were, at the time I noticed them, inexplicable to me; but they now appear dependent on this physical peculiarity of hydrogen. Thus, if a jet of oxygen gas be kindled in an atmosphere of carburetted hydrogen, the flame is smaller than when the converse effect takes place. The voltaic arc between metallic terminals is also much smaller in hydrogen gas than in nitrogen, though both these gases are incapable of combining with the terminals; indeed to obtain an arc at all in hydrogen is scarcely practicable.

Davy has, in his *Researches on Flame*, given several experiments which are similarly explicable; but though noting the results, he nowhere, as far as I am aware, attributes them to any specific peculiarity of hydrogen.

Of the phænomenon which I have examined in this paper, I first published an account in connexion with some experiments on the application of voltaic ignition to lighting mines, and it does not appear impossible that the experiments now detailed may ultimately find some beneficial application in solving the problem of a safety-light for mines. A light which is just able to support itself under the cooling effect of ordinary atmospheric air would be extinguished by air mixed with hydrogenous gas.

I am far from pretending to have devised any means of fulfilling these conditions, and yet supplying an efficient light; I merely throw it out as a suggestion for consideration, knowing that there are no additions to our knowledge which are not ultimately valuable in their practical application; and that a suggestion, however vague,—a new point to those whose minds may be occupied with the subject, may lead them to results which he who makes the suggestion is unable to attain.

P.S. Since this paper was communicated I have received a paper from Dr. Andrews of Belfast, who published as early as 1840, in the *Proceedings of the Royal Irish Academy*, experiments similar to those of mine first published in 1845. My experiments were made in the same year as those of Dr. Andrews, but as I withheld their publication, Dr. Andrews is fully entitled to priority. Had I known of his experiments earlier, I should have recited them in the first part of this paper.

XVI. *Practical application of the Law pointed out by Dr. R. D. Thomson, of the proper Balance of the Food in Nutrition.* By Dr. C. REMIGIUS FRESENIUS, *Professor of Chemistry at the Agricultural Institute of Wiesbaden**.

IN reference to the question concerning the relation which must subsist between the nitrogenous and non-nitrogenous nutritive substances in the food of men and animals, it is but due to Dr. R. D. Thomson to acknowledge, that he considers this the most important circumstance in nutrition, and was the first to call attention to it.

This relation is obviously different in various classes of animals, and besides it must be different even in the same class of animals, according to their mode of life and to the amount of exercise they undergo.

An animal which is hard worked will require a different proportion to one which stands at rest in a stable; still more different must be the proportion when our object is to fatten the animal. I consider it to be one of the most important tasks of dietary and the feeding of cattle, to fix the requisite proportions suited to the various modes of life, for it may be understood that these limits cannot be overstepped on either side without injury.

Let us suppose, for instance, an animal requires under certain circumstances the proportion of 1 nitrogenous (nutritive) to 5 non-nitrogenous (calorifiant) constituents in its food; but if we give it food in which the proportion of 1 to 10 prevails, there will be, in the process of nutrition, for every 1 part nitrogenous only 5 parts non-nitrogenous assimilated; the other half of the non-nitrogenous (calorifiant) aliment will be wasted†.

But it is not the pecuniary loss alone which arises through this, that deserves consideration; for it is clear that the animal will be burdened with the process of getting rid of the unasimilated half; for this object strength is required, which might otherwise have been spared.

If we give it food containing too large a proportion of nitrogenous aliment, in favourable circumstances it will consume the dearer instead of the cheaper non-nitrogenous aliment;

* Translated from the *Lehrbuch der Chemie für Landwirthe, Forstmänner und Cameralisten* von Dr. C. Remigius Fresenius (1847), page 480, by William Augustus Perston.

† The original passage is "So wird es beim Ernährungsprocesse auf je 1 Thl stickstoffhaltige eben doch nur 5 Thle stickstofffreie Bestandtheile verwenden, die andere Hälfte der stickstoffhaltigen Nahrungsmittel wird vergeudet." The true reading it is apprehended ought to be *stickstofffreie Nahrungsmittel*, and it has thus been rendered in the English version.—TRANS.

but in unfavourable circumstances it will become diseased, by being compelled to act in opposition to nature.

Taking it for granted that the requisite proportions for different circumstances were ascertained, the choice of aliment could be regulated on the most rational basis.

[We speak here primarily only of the absolute strength of nourishment, without noticing the greater or less degree of digestibility possessed by equally nutritious substances, and the proportion of unassimilable constituents which they contain.]

We observe, for instance, that cows on a meadow, feeding only upon grass, enjoy good health. Now let us endeavour to ascertain how we can produce the same proportion of non-nitrogenous and nitrogenous aliment with other descriptions of food.

The proportion which exists in grass or hay is 1 to 8·3, as in the following Table:—

	Relation of 1 part nitrogenous to non-nitrogenous.	Relation of 1 part nitrogenous to salts.	The following quantities contain 1 part of nitrogenous matter.			
			Nitrogenous, non-nitrogenous and salts.	Dried at 212°.	Dried in air.	Fresh substances.
	I.	II.	III.	IV.	V.	VI.
French beans	1·81	0·15	2·96	3·45	4·00	
Lentils	1·87	0·09	2·96	3·45	4·00	
Field beans.....	2·08	0·15	3·23	3·66	4·29	
Peas	2·14	0·11	3·25	3·66	4·28	
Wheat.....	2·42	0·11	3·53	4·21	4·85	
Oats	4·08	0·24	5·32	6·41	7·35	
Barley.....	4·25	0·27	5·52	6·53	7·57	
Rye.....	4·42	0·13	5·55	6·29	7·24	
Red turnips	5·08	0·42	6·50	6·45	35·3
Red clover	6·08	0·60	7·68	7·68	9·72	32·0
White turnips.....	6·39	0·55	7·94	7·91	65·1
Indian corn	6·55	0·10	7·65	8·13	9·34	
Mangel-wurzel ...	7·26	0·44	8·70	8·65	48·8
Carrots	7·84	0·55	9·39	9·39	67·6
Meadow-grass ...	8·30	0·73	10·03	10·73	12·47	32·8
Potatoes	9	0·40	10·40	41·2
Oat-straw	12·5	2·04	15·54	40·00	55·55	
Wheat-straw	14·2	2·48	17·68	40·00	54·05	
Rice	14·8	0·10	15·90	16·61	18·41	
Rye-straw	24·4	1·93	27·33	53·48	65·79	
Barley-straw	29·3	3·08	33·38	52·35	58·82	
Cherries	41·	0·18	42·18	175·4
Pears	121·6	0·40	123·00	1250·

This Table, as given by Fresenius, is derived from German authorities, including several results obtained and published by Dr. Thomson in his *Researches on Food*, p. 167. See also *Phil. Mag.*, vol. xxxii. p. 459. There is therefore some discrepancy when compared with English grain, the German grain being richer in nitrogen. See Dr. Thomson on the Composition of German and English Bread, *Phil. Mag.* vol. xxiii. p. 321.

Were we then to give them carrots, in which 1 part nitrogenous is contained for every 7·84 parts of non-nitrogenous constituents, the proportion would not be materially disturbed; but were we to give them potatoes (1 : 9), we disturb the proportion somewhat more. It is therefore expedient to feed them with a substance which is richer in nitrogen; this proper proportion may be obtained with exactness by mixing 1 nutritious equivalent of red clover with 3 nutritious equivalents of potatoes:—

$$1 \times 1 : 6 = 1 : 6$$

$$3 \times 1 : 9 \cdot 00 = 3 : 27$$

$$4 : 33 \text{ or } 1 : 8 \cdot 25.$$

To produce this mixture, we feed them by giving them 9·7 lbs. of dried clover for every 123·6 lbs. of potatoes.

If we wished to give them the same proportion in white turnips and oat-straw, we must supply for every 2 nutritious equivalents of the former 1 nutritious equivalent of the latter; for this mixture gives the proportion of 1 to 8·4; that is, they must be fed with 130 lbs. of fresh white turnips for every 55·55 lbs. of dried oat-straw.

A horse that works hard requires the proportion of 1 to 4. For this we give him oats which represent that proportion. But if we wished to give him the same proportion in field beans and hay, we must take for every 2 alimentary equivalents of the former 1 alimentary equivalent of hay, for such a mixture has the proportion of 1 to 4·1. We feed him therefore with 8·58 lbs. of dry field beans for every 12·47 lbs. of dry hay.

A man requires for a certain mode of life the proportion of 1 to 3. He wishes to eat beef and potatoes; he must, therefore, for every 2 alimentary equivalents of beef eat 1 alimentary equivalent of potatoes, for this mixture gives the proportion of 1 to 3·01; he must therefore use for every 2 lbs. of boiled beef (reckoned without water) 4·1 lbs. of potatoes (reckoned in the fresh state).

If he wished to produce the proportion of 1 to 4 with carrots and raw bacon, he will attain it by mixing 5 alimentary equivalents of the former with 6 alimentary equivalents of the latter, which represent the proportion of 1 to 3·99. For this purpose he must eat 338 parts of fresh carrots for every 11 parts of raw bacon (reckoned free from water).

Concerning the question, as to what is the proper quantity of aliment (possessing the due proportions) which is to be given under different circumstances, experience alone can determine it. For the computation, how the necessary quantity

may be given in diverse properly assorted alimentary mixtures, we would refer to the divisions III., IV., V. and VI. of the foregoing table.

If a cow requires in twenty-four hours 10 kilogrammes (22.05 lbs. avoirdupois) of air-dried hay, how many kilogrammes of the mixture given above of clover and potatoes would it require to replace it?

10 kilogrammes of air-dried clover contain in all 8.04 kilogrammes (17.728 lbs. avoirdupois) of nutritious matter, for

$$12.47 : 10.03 = 10 : x$$

$$x = 8.04.$$

That mixture will consist of 9.7 kilogrammes (21.38 lbs. avoirdupois) of dry clover, which contain in all 7.68 kilogrammes (16.93 lbs.) of nutritious matter and 123.6 kilogrammes (272.5 lbs.) of potatoes, which contain in all 31.20 kilogrammes (68.79 lbs.) of nutritious matter.

133.3 kilogrammes (293.93 lbs.) of the mixture contain accordingly 38.88 kilogrammes (85.72 lbs.) of nutritious matter.

38.88 kilogrammes (85.72 lbs.) of the joint nutritious matters are equal to 133.3 kilogrammes (293.93 lbs.) of the mixture. How many are 8.04 equal to? $x = 27.5$ (60.63 lbs.).

27.5 kilogrammes (60.63 lbs.) of the mixture in question are equivalent to 10 kilogrammes (22.05 lbs.) of hay in the proportion and quantity of nitrogenous and non-nitrogenous alimentary substances. In a precisely similar manner the kind and quantity of the salts must be attended to in practice.

Conclusions from the foregoing.

We have approximated much more closely to the object we had in view, viz. a completely rational system of nutrition, than it has hitherto been possible to do, and can answer the proposed questions with perfectly accurate average numbers; and we have now only duly to consider the influence which the unappropriated portions of food exert on the body (the getting rid of them involves a waste of strength); and further, the greater or less degree of digestibility (*der leichteren oder schwereren, schnelleren oder langsameren Verdaulichkeit*) of each species of aliment, in order to do it with perfect precision.

But we can even now, from what has already been stated, deduce safe and weighty conclusions, namely the following:—

1. It is an impossibility to sustain either a man or a beast on food entirely devoid of nitrogen, however great in quantity it may be.

2. All that has been said in the older as well as in many of the newer books on husbandry, respecting the relative nutritive value of different kinds of forage, cannot, inasmuch as it

was not arrived at by experience, but deduced from theoretical views, possibly be correct, because these views do not accord with facts.

3. The discovery of the true relative value of aliment, and of the proportion in which it may be replaced, may be ascertained without much difficulty, so long as chemists and farmers work hand in hand for the exact solution of the above questions.

4. A completely rational system of nutrition, that is such an one as combines the greatest amount of strength with the least consumption of nourishment, will then be possible.

5. A loss of nutritious matter and of strength often takes place where it would be least expected, namely by the consumption of all kinds of food (or forage) where the due proportion between nitrogenous and non-nitrogenous constituents does not exist, say by eating only fruit or potatoes.

6. It can with safety be decided by the above under what circumstances substitutes for bread may be employed, and what is their respective value for each desired proportion.

Raw and cooked Articles of Food.

Many kinds of food cannot be eaten raw by man; others, although they may be eaten raw, agree much better with us when cooked.

Hence boiling, roasting, baking, &c. has a twofold effect; primarily, it converts indigestible or food difficult of digestion into a digestible or more easily digestible condition. Thus, starch is converted into gelatinous starch, into dextrine or sugar; cartilaginous substances into glue; and chondrine, fibrine, into changed fibrine, &c. Secondly, it frequently confers upon them an agreeable taste.

But can the real nutritive value of food be augmented by cooking? Impossible! Still it may be of the greatest benefit in feeding cattle to cook their food. The advantage accrues in this way: that potatoes, turnips, &c. are more quickly and more easily digested when boiled than raw; and thus there is much less chance for any portion to be thrown off in an undigested state (unassimilated). Its warmth gives also a slight advantage to cooked food; it deprives the body of no heat; and the non-nitrogenous substances, which in the cold food would have been required to afford heat, can be used for the production of fat. But whether cold or warm food is to be preferred in a practical point of view cannot from all this be conclusively deduced. It is a question only to be answered by experience, for the result is entirely dependent on the nature and requirements of the animal.

XVII. *On the alleged Evidence for a Physical Connexion between Stars forming Binary or Multiple Groups, arising from their Proximity alone.* By Prof. J. D. FORBES, F.R.S. &c.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Melrose, July 13, 1849.

IN conformity with usage and with the spirit of your Journal, I may perhaps be permitted to suggest a doubt as to the legitimacy of certain reasonings with respect to the evidence for the *physical connexion* of binary or multiple stars arising from the mere fact of their juxtaposition, as stated and applied by some of the most eminent writers on sidereal astronomy. I should probably have hesitated to oppose my solitary opinion to that entertained by the eminent writers whom I am about to quote, had I not found it to be entirely supported by the eminent authority of two friends to whom I separately proposed it.

Nearly a century ago, Mitchell computed the chances to be 500,000 to 1 against the stars composing the group of the Pleiades being *fortuitously* concentrated within the small apparent space which they occupy; and he thence infers the probability of a physical connexion between them. Struve has pushed this consideration much further. In his classification of double stars he has applied the same argument to estimate the improbability of the occurrence of even *single pairs* of stars in close proximity. He "calculates the odds at 9570 to 1 against any two stars from the 1st to the 7th magnitude inclusive, falling (if fortuitously scattered) within 4" of each other. Now the number of such binary calculations actually observed at the date of this calculation was already 91, and many more have been added to the list. Again, he calculates that the odds against any such stars fortuitously scattered falling within 32" of a third, so as to constitute a triple star, is not less than 173,524 to 1. Now, four such combinations occur in the heavens." Sir John Herschel, from whose *Outlines of Astronomy* I take this statement of Struve's results, adds, "the conclusion of a physical connexion of some kind or other is therefore unavoidable*."

Now I confess my inability to attach any idea to what would be the distribution of stars or of anything else, if "fortuitously scattered," much more must I regard with doubt and hesitation an attempt to assign a numerical value to the antecedent probability of any given arrangement or grouping whatever.

* *Outlines of Astronomy*, p. 564. If I recollect aright, the passage does not occur in the edition in Lardner's *Cyclopædia*.

An equable spacing of the stars over the sky would seem to me to be far more inconsistent with a total absence of Law or Principle, than the existence of spaces of comparative condensation, including binary or more numerous groups, as well as of regions of great paucity of stars. Thus, to take a familiar instance:—No bad representation of stars and their distribution may be made by sparking viscid white paint from a coarse brush upon a dark ground. It is impossible to conceive a nearer approach to a “random scattering.” But I am assured by an ingenious friend, who has used this contrivance in aid of pictorial effect, that such an artificial galaxy will present every variety of grouping, with double and treble points innumerable (as I have indeed myself witnessed); nor can I well see how upon any reasonable theory of chance it should be otherwise.

I wish to restrict this letter to the end proposed, that of nakedly setting forth a serious difficulty in an inferential interpretation of nature, sanctioned by high and also cumulative authority. I shall not therefore attempt now to inquire more minutely into the history of the error, if error it be, nor to insist on the great importance of arguing correctly in cases which admit of so very extensive application.

I remain, Gentlemen,

Yours faithfully,

JAMES D. FORBES.

XVIII. *On Quaternions; or on a New System of Imaginaries in Algebra.* By Sir WILLIAM ROWAN HAMILTON, LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, &c., Andrews’ Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.

[Continued from vol. xxxiv. p. 439.]

82. **T**HIS seems to be a proper place for inserting some notices of investigations and results, respecting the inscription of rectilinear (but not generally plane) polygons, in spheres, and other surfaces of the second degree.

Let ρ and σ be any two unit-vectors, or directed radii of an unit-sphere; so that, according to a fundamental principle of the present Calculus, we may write

$$\rho^2 = \sigma^2 = -1. \quad . \quad . \quad . \quad . \quad (237.)$$

We shall then have also,

$$0 = \sigma^2 - \rho^2 = \sigma(\sigma - \rho) + (\sigma - \rho)\rho, \quad . \quad . \quad . \quad (238.)$$

and consequently

$$\sigma = -(\sigma - \rho)\rho(\sigma - \rho)^{-1} = -\lambda\rho\lambda^{-1}, \quad \dots (239.)$$

if λ be the directed chord $\sigma - \rho$ itself, or any portion or prolongation thereof, or any vector parallel thereto. If then $\rho, \rho_1, \rho_2, \dots \rho_n$, be any series or succession of unit-vectors, while $\lambda_1, \lambda_2, \dots \lambda_n$ are any vectors respectively coincident with, or parallel to, the successive and rectilinear chords of the unit-sphere, connecting the successive points where the vectors $\rho \dots \rho_n$ terminate; and if we introduce the quaternions,

$$q_1 = \lambda_1; \quad q_2 = \lambda_2\lambda_1; \quad q_3 = \lambda_3\lambda_2\lambda_1; \quad \&c., \quad \dots (240.)$$

we shall have the expressions,

$$\rho_1 = -q_1\rho q_1^{-1}; \quad \rho_2 = +q_2\rho q_2^{-1}; \quad \rho_3 = -q_3\rho q_3^{-1}; \quad \&c. \quad (241.)$$

Hence if we write the equation

$$\rho_n = \rho, \quad \dots \dots \dots (242.)$$

to express the conception of a *closed* polygon of n sides, inscribed in the sphere, we shall have the general formula,

$$\rho q_n = (-1)^n q_n \rho; \quad \dots \dots \dots (243.)$$

which is immediately seen to decompose itself into the two following principal cases, according as the number n of the sides is even or odd:

$$\rho q_{2m} = +q_{2m}\rho; \quad \dots \dots \dots (244.)$$

$$\rho q_{2m+1} = -q_{2m+1}\rho. \quad \dots \dots \dots (245.)$$

The equation (244.) admits also of being written thus, by the general rules of quaternions,

$$0 = V.\rho V q_{2m}; \quad \dots \dots \dots (246.)$$

and the equation (245.) resolves itself, by the same general rules, into the two equations following:

$$0 = S q_{2m+1}; \quad 0 = S. q_{2m+1} \rho. \quad \dots \dots \dots (247.)$$

We shall now proceed to consider some of the consequences which follow from the formulæ thus obtained.

83. An immediate consequence of the equations (247.), or rather a translation of those equations into words, is the following quaternion theorem:—*If any rectilinear polygon, with any odd number of sides, be inscribed in a sphere, the continued product of those sides is a vector, tangential to the sphere at the first corner of the polygon.* It is understood that, in forming this continued product of sides, their *directions* and *order* are attended to; the first side being multiplied as a vector by the second, so as to form a certain quaternion product; and this product being afterwards multiplied, in succession, by the third side, then by the fourth, the fifth, &c., so as to form a

series of quaternions, of which the *last* will (by the theorem) have its *scalar* part equal to zero; while the *vector* part, or the product itself, will be constructed by a right line with a certain definite direction, which will (by the same theorem) be that of a certain rectilinear tangent to the sphere, at the point or corner where the first side of the inscribed polygon begins. [The *tensor* of the resulting vector, or the *length* of the product line, will of course represent, at the same time, by the general law of tensors, the product of the lengths of the factor lines, with the usual reference to some assumed unit of length.] And conversely, whenever it happens that an odd number of successive right lines in space, being multiplied together successively by the rules of the present Calculus, give a *line* as their continued product, that is to say, when the scalar of the quaternion obtained by this multiplication vanishes, then those right lines may be inferred to have the *directions* of the successive sides of a polygon inscribed in a sphere.

84. Already, even as applied to the case of an inscribed *gauche pentagon*, the theorem of the last article expresses a *characteristic* property of the *sphere*, which may be regarded as being of a *graphic* rather than of a *metric* character; inasmuch as it concerns immediately *directions* rather than *magnitudes*, although there is no difficulty in deducing from it metric relations also: as will at once appear by considering the formula which expresses it, namely the following,

$$0 = S. (\rho - \rho_4)(\rho_4 - \rho_3)(\rho_3 - \rho_2)(\rho_2 - \rho_1)(\rho_1 - \rho). \quad (248.)$$

(See the Proceedings of the Royal Irish Academy for July 1846, where this quaternion theorem for the case of the inscribed pentagon was given.) For the theorem assigns, and in a simple manner expresses, to those who accept the language of this Calculus, a relation between the *five* successive directions of the sides of a *gauche pentagon* inscribed in a sphere, which appears to the present writer to be *analogous* to (although necessarily more complex than) the angular relation established in the third book of Euclid's Elements, between the *four* directions of the sides of a plane quadrilateral inscribed in a circle. Indeed, it will be found to be easy to deduce the property of the plane inscribed quadrilateral, from the theorem respecting the inscribed *gauche pentagon*. For, by conceiving the fifth side $\rho_4\rho$ of the pentagon $P \dots P_4$ to tend to vanish, and therefore to become tangential at the first corner P , it is seen that the vector part of the quaternion which is the continued product of the four first sides must tend, at the same time, to become normal to the sphere at P ; in order that, when multiplied into a tangential vector there, it may

give a vector as the product. Hence the vector part of the product of the four successive sides of an inscribed gauche quadrilateral $PP_1P_2P_3$, is constructed by a right line which is normal to the sphere at the first corner; and more generally, either by the same geometrical reasoning applied to the theorem of art. 83, or by considering the signification of the formula (246.), we may deduce this other theorem, that *the vector of the continued product of the successive sides of an inscribed gauche polygon $P \dots P_{2m-1}$, of any even number of sides, is normal to the sphere at the first corner P .* Suppose now the inscribed quadrilateral, or more generally the polygon of $2m$ sides, to flatten into a plane figure; it will thus come to be inscribed in a circle, and consequently in infinitely many spheres at once; and the only way to escape a resulting indeterminateness in the value for the vector of the product, is by that vector vanishing: which accordingly it may be otherwise proved to do, although the present mode of proof will appear sufficient to those who examine its principles with care. And thus we shall find ourselves conducted to the well-known graphic property of the quadrilateral inscribed in the circle, and more generally to a corresponding theorem respecting inscribed hexagons, octagons, &c., under the form of the following proposition in quaternions, which expresses a characteristic property of the circle:—*The vector part of the product of the successive sides of any polygon, with any even number of sides, inscribed in a circle, vanishes; or, in other words, the product thus obtained, instead of being a complete quaternion, reduces itself simply to a positive or negative number.* On the other hand, it is easy to see, from what precedes, that *the product of the successive sides of a triangle, pentagon, or other polygon of any odd number of sides, inscribed in a circle, is a vector, which touches the circle at the first corner of the polygon, or is parallel to such a tangent.*

85. Although the precise law of the relation between the directions of the sides of an inscribed gauche pentagon, heptagon, &c., expressed by the first formula (247.), is peculiar to the sphere; yet it is easy to abstract from that relation a part, which shall hold good, as a law of a more general character, for other surfaces of the second order. For we may easily infer, from that formula, especially when combined with the other equations of art. 82, that *if the first $2m$ sides of an inscribed polygon of $2m$ sides, $P'P'_1P'_2 \dots P'_{2m}$, be respectively parallel to the successive sides of another polygon of $2m$ sides, $PP_1 \dots P_{2m-1}$, inscribed in the same surface, then the last side, $P'_{2m}P'$, of the former polygon, will be parallel to the plane which touches the surface at the first corner P of the latter polygon:*

and under *this* form of enunciation, it is obvious that the theorem must admit of being extended, by deformation, to ellipsoids, and other surfaces of the second degree. We may then enunciate also this other theorem, respecting the inscription of rectilinear polygons in such surfaces (which theorem was communicated to the Royal Irish Academy in March 1849): —*If, after inscribing, in a surface of the second degree, any gauche polygon of $2m$ sides, $PP_1 \dots P_{2m-1}$, we then inscribe in the same surface another gauche polygon, of $4m+1$ sides, $P'P'_1 \dots P'_{4m}$, under the following $4m$ conditions of parallelism:*

$$P'P'_1 \parallel PP_1; \quad P'_1P'_2 \parallel P_1P_2; \quad \dots \quad P'_{2m-1}P'_{2m} \parallel P_{2m-1}P; \quad (249.)$$

and

$$P'_{2m}P'_{2m+1} \parallel PP_1; \quad P'_{2m+1}P'_{2m+2} \parallel P_1P_2; \quad \dots \quad P'_{4m-1}P'_{4m} \parallel P_{2m-1}P; \quad (250.)$$

(the first corner P' of the second polygon being assumed at pleasure on the surface, and the other corners P'_1 , &c., of that polygon, being successively derived from this one, by drawing two series of parallels as here directed;) *then the diagonal plane $P'P'_{2m}P'_{4m}$, which contains the first, middle, and last corners of the polygon with $4m+1$ sides, will be parallel to the plane which touches the surface at the first corner P of the polygon with $2m$ sides.* In fact, the two rectilinear diagonals, $P'P'_{2m}$ and $P'_{2m}P'_{4m}$, will, by a former theorem of the present article, be parallel to that tangent plane. For example, if the first, second, third, and fourth sides, of a gauche quadrilateral inscribed in a surface of the second order, be parallel to the first, second, third, and fourth, and also to the fifth, sixth, seventh, and eighth sides respectively, of a gauche enneagon inscribed in the same surface; then that diagonal plane of the enneagon which contains the first, fifth, and ninth corners thereof, will be parallel to the plane which touches the surface at the first corner of the quadrilateral.

[To be continued.]

XIX. *On the Weather during the Quarter ending June 30, 1849.* By JAMES GLAISHER, Esq., F.R.S., and of the Royal Observatory, Greenwich*.

DURING the past quarter I have inspected the locality and the instruments at Exeter, Southampton, Latimer, Aylesbury, Stone, Hartwell House, Hartwell Rectory, Oxford, Cardington, Liverpool, Leeds, Stonyhurst, York, Whitehaven and Newcastle.

The results furnished to the Registrar-General for the past

* Communicated by the Author.

quarter are mostly satisfactory; these I have as usual examined and reduced.

The daily temperatures of the air till April 28 and after June 6 were for the most part below their average values; the mean amount of deficiency in the former period was $3^{\circ}7$, and in the latter it was $2^{\circ}7$. The daily temperatures between April 29 and June 5 were alternately in excess and defect; the mean temperature of the interval was somewhat above its average value. The days whose mean temperatures departed the most from their averages were April 17, 19, 20, 21; May 10 and June 12; the defect in these cases were $11^{\circ}2$; $14^{\circ}5$; $10^{\circ}4$; $11^{\circ}1$; $10^{\circ}2$; and $10^{\circ}7$ respectively. The mean temperature of the three months ending May, constituting the three spring months, was $46^{\circ}6$, and that of the average of the seventy preceding springs is $46^{\circ}7$. The several subjects of research in the past quarter are detailed below.

The mean temperature of the air for the month of April was $43^{\circ}2$, being less than the average of seventy years by $2^{\circ}7$, and less than the average of the preceding eight years by $5^{\circ}0$.

For the month of May was $54^{\circ}0$, exceeding the average for seventy years by $1^{\circ}2$, and being less than the average of the preceding eight years by $0^{\circ}4$. The mean of the quarter was $51^{\circ}7$, being $0^{\circ}5$ less than the average of seventy years, and $2^{\circ}1$ less than the average of the preceding eight years.

For the month of June was $57^{\circ}9$, being of the same value as that of the average from seventy years, and being less than that of the preceding eight years by $1^{\circ}9$.

Generally the differences of temperature at one place from the average values for that place sufficiently indicate the departures from the means for all other places; but in the past three months this has not been the case, the northern part of the country having been subjected to a longer continuance of low temperature than usual, and the departures from the mean temperatures in the northern parallels of latitude have been greater than in the southern. This will be more clearly seen in the following table.

Year.	Mean Temperature of the Air in the Quarter ending June 30.				
	In Cornwall and Devonshire.	South of Latitude 52° .	Between the Latitudes of 52° and 53° .	Between the Latitudes of 53° and 54° .	North of 54° .
1847	$51^{\circ}3$	$51^{\circ}9$	$51^{\circ}2$	$52^{\circ}0$	$48^{\circ}4$
1848	$54^{\circ}1$	$54^{\circ}0$	$53^{\circ}6$	$52^{\circ}0$	$50^{\circ}7$
1849	$52^{\circ}0$	$52^{\circ}1$	$50^{\circ}0$	$49^{\circ}9$	$49^{\circ}8$

The mean temperature of evaporation at Greenwich—

For the month of April was $41^{\circ}5$; for May was $49^{\circ}0$; and for June was $48^{\circ}7$. These values are $2^{\circ}6$, $1^{\circ}7$ and $6^{\circ}7$ below, respectively, the averages of the same months in the preceding eight years.

The mean value for the quarter was $46^{\circ}4$, which is $3^{\circ}6$ below the average of corresponding quarters of eight years.

The mean temperature of the dew-point at Greenwich—

For the months of April, May and June, were $39^{\circ}1$, $43^{\circ}9$, and $48^{\circ}4$ respectively. These values are $1^{\circ}8$, $3^{\circ}9$ and $4^{\circ}0$ below, respectively, the averages of the same months in the preceding eight years.

The mean value for the quarter was $43^{\circ}8$, which is $3^{\circ}2$ below the average from the preceding eight years.

The mean elastic force of vapour for the quarter was 0.342 inch, which is 0.038 inch less than the average for the preceding eight years.

The mean weight of water in a cubic foot of air for the quarter was 3.5 grains, which is 0.3 grain less than the average for the preceding eight years.

The mean additional weight of water required to saturate a cubic foot of air was 1.2 grain.

This value for the preceding eight years was 1.1 grain.

The mean degree of humidity in April was 0.864, in May was 0.703, and in March was 0.715. The averages for the eight preceding years were 0.802, 0.797, and 0.778.

The mean reading of the barometer at Greenwich in January was 29.517 inches, in February was 29.766 inches, and in March was 29.868 inches. These values are respectively 0.215 inch less, 0.022 inch less, and 0.080 inch greater than the averages of the same months for the preceding eight years.

The average weight of a cubic foot of air under the average temperature, humidity and pressure, was 534 grains; the average for the eight preceding years was 535.4 grains.

The rain fallen at Greenwich in April was 2.2 inches; in May was 3.9 inches; and in June was 0.2 inch. The amount for the quarter was 6.3 inches; the average amount for the preceding eight years was 4.74 inches. This excess of rain was experienced only in the southern part of England. The fall of rain between the latitudes of 52° and 53° was about the average for this parallel. North of 53° the fall has been small, and but little more than half the usual amount.

The horizontal movement of the air has been less than usual; its direction is uncertain; observers in the same locality have deduced the average direction differently.*

* Within the last few weeks a system of daily returns of the direction of the wind, taken simultaneously at many different places in England, the

The average daily ranges of the thermometer in air, at the height of four feet, were $16^{\circ}0$, $16^{\circ}3$, and $20^{\circ}6$. The average ranges of these three months from the observations of the eight preceding years, were $16^{\circ}8$, $19^{\circ}2$, and $19^{\circ}2$ respectively.

The readings of the thermometer on grass in April was at and below 32° on nineteen nights, the lowest was 19° ; between 32° and 40° on eight nights, and above 40° on three nights. In May the lowest reading was $26^{\circ}7$; and the readings were below 32° on four nights; between 32° and 40° on seven nights; and above 40° on twenty nights. In June the reading was 32° on one night; at and below 40° on nine nights; between 40° and 50° on eighteen nights; and above 50° on one night.

At St. John's Wood the lowest reading of a thermometer with its bulb placed in a parabolic reflector and fully exposed to the sky, was $20^{\circ}2$ in April, $27^{\circ}2$ in May, and 31° in June.

At Cardington the reading of a thermometer on grass was less than 32° on eight nights in June; the lowest reading was $26^{\circ}5$.

At Wakefield, on June 12 and 13, water exposed to the sky was frozen on both nights.

At Whitelhaven the month of June was unusually cold, and vegetation was subjected to very low temperatures at night; a thermometer placed on grass on a layer of wool frequently fell many degrees below the freezing-point, and on two nights it fell to 25° . Ice was seen on several mornings, and snow fell amongst the mountains on the 3rd, a phænomenon which has not been witnessed in June since the year 1827.

During the month of June the readings at night were unusually low, even the temperature of the air in many places nearly fell to 32° , and actually did so in York, which was a point lower than the observer Mr. Ford had ever before seen in June.

There were four exhibitions of the *aurora borealis* during the quarter ending June 30, 1849; it occurred on May 31, and was seen at Stone; on June 15 at Stone; on June 26 at Latimer, auroral streamers passed from S.W. nearly across the zenith to the horizon in the N.E.; and on June 30, at Latimer, some beautiful auroral flashes were seen at 10^h 30^m P.M.

Thunder-storms occurred on April 28 and on May 2 at Stone; on May 3 at Uckfield, Stone, Saffron Walden, and Nottingham; on May 4 at Uckfield; on May 14 at Saf-

south of Scotland, and which it is to be hoped will soon embrace some portions of Ireland, have been organized. All the stations from which returns are now sent, I have visited and given instructions to the observers so as to ensure accuracy. The observations are published daily in the *Daily News* newspaper. If this system be continued some time valuable information will be collected.

from Walden, Holkham, and Nottingham; on May 17 and 18 at Leicester and Nottingham; on May 22 at Nottingham; on June 4 at Uckfield and Nottingham; on June 5 at Norwich; on June 6 at Holkham; on June 7 at Helston; on June 9 at Helston; on June 16 at Hartwell; and on June 17, 18 and 19, at Helston.

Lightning was seen but thunder was not heard on May 3, at Uckfield; on May 4 at Uckfield, Nottingham and Stone; on May 14 at Leicester; on June 3 at Uckfield; on June 4 at Wakefield; on June 5, 7 and 9, at Helston; and on June 12 at Nottingham.

Thunder was heard but lightning was not seen at Wakefield on April 6; at Exeter on May 5; at Uckfield on June 4 and 8; at Hartwell on May 3, 14, 15, 18, and June 6; and at Norwich on June 28.

Hail fell at Hartwell on April 2; at Manchester on April 11; at Truro and Saffron Walden on April 13; at Hartwell on April 14; at Truro and Saffron Walden on April 17 and 18; at Truro on the 19th; at Truro, Saffron Walden, Hartwell and Holkham on April 20; at Holkham on April 21; at Exeter on May 5; at Holkham on May 14; at Helston on June 7 and 9; at Hartwell on June 16; and at Helston on June 17 and 19.

Snow fell at various places on April 13, 16, 17, 18, 19, 20, and 21.

Solar halos were seen at different places on April 2, 8, 25, 27; May 12, 13, 19, 29, 30, 31; June 15, 18, 23 and 24. This unusual number of solar halos indicates a very unusual prevalence of the cirrostratus cloud during the day. A lunar halo was seen at Hartwell on May 31.

The reading of the barometer on April 1 was 29·4 inches; it decreased to 29·3 inches on the 2nd, and increased to 29·55 on the 3rd. On the 4th it decreased quickly, and was 29·28 on the morning of the 5th. From this time to the 8th the change of reading was small. On the 9th the reading was 29·34; it began to increase, and was 29·71 on the morning of the 12th, when it began to decrease rapidly, and it was 29·09 during the afternoon of the 13th. On the 14th it increased slowly, and on the 15th it was 29·6. The reading continued about this value till the morning of the 18th, when it was 29·77; it then began to decrease quickly, and on the morning of the 19th was 29·08, which was the lowest during the month. During the remainder of the 19th and till the evening of the 21st the reading increased, and was 29·83 at the latter time. The reading decreased on the 22nd, and was 29·48 on the 23rd. From the 24th to the 28th it was about 29·6, and then increased to 30·15 on the 29th, and to 30·18 on the morning

of the 30th. This reading was the highest in the month; but it soon again decreased, and before midnight descended below 30 inches. The range during this month was 1·09 inch.

On May 1 the reading was 29·92, which decreased to 29·63 on the 5th; increased to 29·86 on the 9th; it then decreased to 29·72 by the evening of the 11th, increased to 30·09 by the 12th, and decreased to 29·18 on the 17th; this was the lowest reading during the month. The reading, with slight exceptions, increased till the 24th, when it was 30·07; it then decreased and increased alternately, but the changes were small till the end of the month. The highest reading was 30·08, and it took place on the 29th. The range within this month was 0·90 inch.

During the month of June the changes of reading were small. The lowest reading was 29·63 on the 16th, and the highest was 30·06 on the 22nd. The range therefore within the month was 0·43 inch only.

The following are the agricultural reports with which I have been favoured.

At Guernsey, the particulars having been furnished by Dr. Hoskins, F.R.S.

In April, from the 10th to the 20th, there were cold winds with showers of hail and sleet which checked the forward vegetation and destroyed crops of early potatoes. In May, fogs with high temperatures were prevalent, and there were frequent light gales and heavy showers of rain. Vegetation generally recovered from the checks it received in April; grass and other crops were luxuriant, asparagus fine and abundant; wall-fruit, horse-chestnuts, sycamores, and other trees of early foliage, in exposed situations were much injured by blight.

In the early part of June there were frequent thunder-storms, with fine sultry weather. Towards the end of the month fogs were prevalent; there was an unusual prevalence of easterly winds; strawberries were abundant and well-flavoured, crops of grass luxuriant, as well as other vegetation, notwithstanding the paucity of rain.

At Uckfield, the particulars having been furnished by C. L. Prince, Esq.

On April 19 very heavy rain fell early in the morning; at 3 P.M. on this day the wind shifted suddenly to N.E., and a severe gale and heavy snow continued for eight hours; as the temperature of the air at the time fell to 32°, it almost destroyed the gooseberry bloom, as well as that of the early cherries. The wall-fruit was much injured, and in some places the trees were killed, being cased with ice during the night, and thawed suddenly by the sun on the 20th.

At Leeds, by Charles Charnock, Esq.

The cold and dry parching winds are seriously affecting spring-sown crops on dry soils. The Swedish turnips are a very patchy crop and have been resown in many places. Barley and oats are short. Beans are affected with the *Aphis*. Wheat on strong soils is very deficient, but on light soils it is better. Potatoes have been much cut down by the white frosts. On the whole the country is suffering much from the want of rain. Cattle and sheep are healthy. Employment for agricultural labourers is scarce, and as a body they are suffering severely.

Hampshire, the particulars having been furnished by John Clark, Esq. of Timsbury Farm, near Romsey.

The prospect for those farmers who have been in positions to do justice to their operations, is cheering; every crop promises to be abundant. The hay harvest is nearly completed, and in the most satisfactory manner. There have however been many instances of the truth of the old saying, that more hay is spoiled in fine weather than in catching seasons. The vigorous and thick growth of the grass has required more time to perfect than many farmers have allowed, and injury has resulted.

The crops on badly-farmed lands are thin and poor, oats are generally indifferent. The season for turnip tillage has been all that could be desired, excepting on neglected stiff lands, and there the needful pulverization of the land has not been obtained for want of moisture. Now, July 6, every description of root-crop is languishing for want of rain. The turnip-fly has not been so troublesome as in past seasons. Potatoes appear generally healthy. There is every prospect of a full average yield of wheat, and should there be a continuance of fine weather, it will be gathered much earlier than usual.

At Stonyhurst, the particulars having been furnished by the Rev. Alfred Weld, F.R.A.S.

The lambing season began in this neighbourhood on the 23rd of March, and continued three weeks; there were several losses in the country owing to the severity of the weather. During April and the early part of May the weather was remarkably dry and unfavourable to the growth of grass; this, added to the general dryness of the season, has caused the hay crop to be very late. The season for sowing was very favourable; oats were sown first on March 23, and now make a fine show. Early potatoes planted before April 1 are growing well and without any signs of disease. The crop is very abundant, frequently producing 20 to 30 to a root. Later potatoes planted in April escaped the effects of the frosts, which are said to have destroyed a considerable portion of the crops

in the neighbourhood of Manchester and Liverpool. Oats have been infested with charlock to a great degree. The sowing of beet began May 5; the crop is healthy and forward. Turnips planted about May 10 are very luxuriant and promising; no fly has appeared. Sheep-washing took place about May 21, which is about the usual time in this part of the country. Some ewes shorn on June 14 died from cold. On June 9 vegetation appeared to stop from the cold weather, which continued till the 15th. The showers which brought on the green crops so well were not sufficient for the grass, which is still very short, though full in the root. The hay season generally begins in this part about June 26, whereas this year (now July 8) none is cut except one or two small patches; the grass is still growing fast, and promises to be an abundant crop. Wheat has been in ear about twelve days; oats are just opening out.

Nottingham, the particulars furnished by E. J. Lowe, Esq, F.R.A.S.

Wheat, barley and oats look well; the grass crops are heavy, and potatoes promise well. The frost of April 18 did great injury to fruit.

The monthly values of the several subjects of research appear in the Registrar-General's Quarterly Report; the quarterly values are shown in the subjoined table:—

The mean of the numbers in the first column of this table is 29·618 inches, and this value may be considered as the pressure of dry air for England during the quarter ending June 30, 1849.

The mean of the numbers in the second column, for Guernsey and those places situated in the counties of Cornwall and Devonshire, is $52^{\circ}0$; for those places situated south of latitude of 52° , including Chichester and Hartwell, is $52^{\circ}1$; for those places situated between the latitudes of 52° and 53° , including Saffron Walden and Leicester, is $50^{\circ}0$; for those places situated between the latitudes of 53° and 54° , including Derby and York, is $49^{\circ}9$; and for Whitehaven and Newcastle is $49^{\circ}8$. These values may be considered as those of the mean temperatures of the air for these parallels of latitude during the quarter ending June 30, 1849.

The average daily range of temperature in Cornwall and Devonshire was $14^{\circ}8$; at Liverpool and Whitehaven was $12^{\circ}1$; south of latitude 52° was $19^{\circ}9$; between the latitudes of 52° and 54° was $17^{\circ}6$; and at Whitehaven and Newcastle was $17^{\circ}6$.

The greatest mean daily ranges of the temperature of the air took place at St. John's Wood, Latimer, Aylesbury, and Beckington: that at St. John's Wood is very large; is it right?

and the least occurred at Truro, Liverpool, Guernsey, and Whitehaven.

The highest thermometer readings during the quarter were 88° at Southampton, 86° at Walworth, 85° at St. John's Wood and at Latimer. The lowest thermometer readings were 24° at Leicester, $24^{\circ}3$ at Highfield House, and 25° at Uckfield and at Aylesbury. The extreme range of temperature of the air during the quarter in England was therefore about 61° , considering the true extremes as 24° and 85° .

The average quarterly range of the reading of the thermometer in Cornwall and Devonshire was $43^{\circ}0$; at Liverpool and Whitehaven was $41^{\circ}5$; south of latitude 52° was $55^{\circ}5$, and north of 52° was $51^{\circ}1$.

The mean temperature of the dew-point in Cornwall and Devonshire was $43^{\circ}7$; south of latitude 52° was $43^{\circ}5$; between 52° and 53° was $42^{\circ}2$, and north of 53° was $43^{\circ}6$.

The amount of cloud seems to have been less than usual.

Rain has fallen on the greatest number of days at Hartwell, Wakefield, and Cardington. The average number at these places was 54. It fell on the least number of days at Maidenstone Hill, Hereford and Beckington, and the average number at these places was 33. The stations at which the largest falls have taken place are Truro, Newcastle and Helston. The smallest falls occurred at York, and generally in the north of England. The average fall in the counties of Cornwall and Devonshire was 8.1 inches; south of latitude 52° was 6.4 inches; between the latitude of 52° and 53° was 7.4 inches; between 53° and 54° was 5.5 inches; and at Newcastle and Whitehaven was 7.8 inches.

The numbers in the columns 15 to 18 show the mean values of the hygrometrical results; from which we find that—

The mean weight of vapour in a cubic foot of air at all places (excepting Cornwall and Devonshire) in the quarter ending June 30, 1849, was 3.5 grains.

The mean additional weight required to saturate a cubic foot of air in the quarter ending June 30, 1849, was 0.9 grain.

The mean degree of humidity (complete saturation = 1) in the quarter ending June 30, 1849, was 0.776.

The mean amount of vapour mixed with the air would have produced water, if all had been precipitated at one time on the surface of the earth, to the depth of 4.2 inches.

The mean weight of a cubic foot of air under the mean pressure, temperature and humidity, was 532 grains at the average height of 170 feet

And these values for Cornwall and Devonshire were 3.5 grains; 0.9 grain; 0.749; 4.3 inches; and 534 grains, at the average height of 120 feet.

Meteorological Table for the Quarter ending June 30, 1849.

Names of the places.	Mean pressure of dry air reduced to the level of the sea.	Mean the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Mean monthly range.	Range of temperature in the quarter.	Mean temperature of the dew-point.	Wind.		Mean amount of cloud.	Rain.		Mean weight of vapour in a cubic foot of air.	Mean degree of humidity.	Mean whole amount of column of atmosphere.	Mean weight of a cubic foot of air.	Height of station of the barometer above the level of the sea.
									Mean estimated strength.	General direction.		Number of days on which it fell.	Amount collected.					
Guernsey	in.	53.9	75.0	36.0	11.5	25.2	39.0	43.0	..	e. by n.	..	36	7.2	gr.	0.705	4.2	grs.	feet.
Helson	29.591	51.6	76.0	30.0	14.6	36.7	46.0	46.0	1.4	s.w.	4.9	38	9.2	0.4	0.881	4.6	533	123
Falmouth	51.7	78.0	32.0	18.4	37.3	46.0	..	1.6	n. & var.	6.2	43	8.1	0.8	535	106
Truro	50.6	69.9	35.0	11.1	26.3	34.0	n.	5.8	39	10.2
Exeter	29.708	52.3	78.0	28.0	18.4	38.3	50.0	42.2	1.8	e. & w.	4.7	37	7.5	1.4	0.711	4.0	535	140
Chichester	51.8	76.0	28.0	15.4	35.0	48.0	41.5	..	s.	..	38	7.3	1.7	0.666	4.0	533	180
Uckfield	29.737	54.0	84.0	25.0	20.3	43.7	59.0	42.0	..	s.w.	6.6	36	7.0	1.2	0.757	4.7	533	55
Southampton	29.603	55.1	88.0	28.8	20.7	43.4	59.2	47.2	0.5	n.e.	6.7	35	5.8	0.9	0.620	3.4	534	265
Beckington	29.747	51.5	79.0	25.0	21.0	44.0	54.0	36.8	1.0	s.w.	7.0	32	7.0	0.8	0.822	4.6	541	107
Maidenstone Hill, Greenwich ..	29.587	51.6	83.2	26.9	17.6	38.0	56.3	46.0	..	n.	..	39	6.3	1.2	0.761	4.2	534	159
Royal Observatory, Greenwich ..	29.605	51.0	80.7	26.8	16.0	39.4	53.9	43.8	..	n.	7.0	32	7.0	0.7	0.859	4.9	535	..
St. John's Wood	29.511	53.2	85.0	27.0	29.0	44.8	58.0	48.6	1.2	Variable.	7.0	51	5.9	0.7	32
Walworth	52.6	86.0	27.0	17.3	39.6	59.0	..	2.6	s.w.	7.6	46	5.5	0.9	530	335
Latimer Rectory, Bucks	29.589	51.3	85.0	26.0	21.5	44.8	59.0	44.5	1.5	n.	6.4	46	7.1	0.9	0.798	4.3	529	280
Aylesbury	29.557	52.3	82.0	25.0	21.1	46.7	57.0	42.7	0.5	s.e.	6.9	48	7.3	1.3	0.723	4.1	532	320
Stone Observatory	29.658	50.3	77.7	26.5	18.1	41.0	51.2	42.6	1.0	n.e.	6.6	51	7.5	1.1	0.771	4.0	531	260
Hartwell House, Bucks	51.2	80.0	29.0	20.9	46.0	51.0	42.8	0.8	n.	6.7	57	6.3	1.0	0.759	4.0	531	260
Hartwell Rectory	50.9	43.3	..	Variable.	3.3	49	..	1.0	0.763	4.1	531	260
Saffron Walden	50.7	76.0	28.0	16.5	35.3	48.0	..	2.1	Variable.	4.5	38	5.3	250
Radcliff Observatory, Oxford	51.5	n.e.	7.8	33	8.8
Hereford	48.5	n.e.	6.9	33	8.8
Cardington	29.558	51.0	77.0	28.6	17.9	39.6	48.4	43.8	..	Variable.	6.9	52	5.3	1.0	0.782	4.2	536	70
Norwich	29.589	49.9	78.0	22.0	17.7	37.3	56.0	45.0	..	Variable.	..	48	8.9	0.7	0.850	4.4	538	39
Holkham	29.580	49.6	74.6	25.8	14.5	35.7	48.8	43.2	..	n.e.	6.5	50	7.9	0.7	0.834	4.3	538	31
Leicester	29.755	48.5	81.0	24.0	19.8	46.0	57.0	36.9	..	Variable.	5.4	42	8.2	1.3	0.703	3.4	538	156
Derby	49.5	73.0	25.0	17.0	39.0	48.0	44.8	..	Variable.	..	53	7.7	0.6	0.855	4.3	533	39
Highfield House, Notts	29.653	50.5	79.0	24.3	19.0	43.4	54.7	44.9	1.3	n.w.	6.6	50	7.2	0.8	0.811	4.5	536	103
Manchester	51.4	n.w.	6.0	41	5.4	0.9	0.798	4.2	538	37
Liverpool	29.602	50.5	84.0	26.0	18.7	48.0	58.0	42.9	..	w.	..	54	4.3	0.9	0.773	4.0	536	113
Wakfield Prison	29.608	50.6	79.0	27.0	19.5	46.0	52.0	42.3	..	w.	5.2	47	3.7	1.1	0.758	4.0	537	148
Holmfeld House	50.0	77.0	27.0	17.5	43.0	50.0	43.8	1.3	n.e. & s.w.	4.8	50	5.7	0.8	0.811	4.2	537	381
Stourthurst Observatory	29.033	48.2	74.2	25.3	17.8	39.2	48.9	41.9	1.0	n.e. & s.w.	6.7	48	5.8	0.8	0.809	4.0	536	50
York	48.6	73.0	27.0	15.7	39.0	46.0	n.e.	..	40	4.0
Whitehaven	29.555	51.1	70.0	29.0	13.0	31.2	41.0	41.7	2.1	s.w.	..	39	5.7	1.2	0.722	4.0	535	80
Newcastle	29.553	48.4	71.0	26.0	13.5	32.5	45.0	42.9	..	s.e. & n.e.	..	39	9.9	0.7	0.834	4.1	537	121
Number of columns	1	2	3	4	5	6	7	8	9	10	13	1.5	16	17	18	19

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 71.]

March 29, "GENERAL Methods in Analysis, for the resolution of Linear Equations in Finite Differences and Linear Differential Equations." By Charles James Hargreave, Esq., LL.B., F.R.S. &c.

The investigations presented in this paper consist of two parts; the first offers a solution, in a qualified sense, of the general linear equation in finite differences; and the second gives an analysis of the general linear differential equation with rational factors, so far as concerns its solution in series.

The author observes that there does not at present exist any general method of solving linear equations in finite differences of an order higher than the first; and that with reference to such equations of the first order, we obtain insufficient forms which are intelligible only when the independent variable is an integer. It is in this qualified sense that the solutions proposed in this paper are to be taken; so that the first part of these investigations may be considered as an extension of this form of solution from the general equation of the first order to the general equation of the n th order.

In the second part, the author points out a method by which the results of the process above indicated may be made to give solutions of those forms of linear differential equations whose factors do not contain irrational or transcendental functions of the independent variable, or contain them only in an expanded form.

This object is effected by means of the theorem, relative to the interchange of the symbols of operation and of quantity, propounded by the author in a former memoir published in the Philosophical Transactions (Part I. for 1848, p. 31). It is one of the properties of this singular analytical process that it instantaneously converts a linear equation in finite differences into a linear differential equation; so that whenever the former is soluble, the latter is soluble also, provided the result be interpretable; a condition satisfied when the functions employed are rational algebraical functions.

Notwithstanding the qualified character of the solutions previously obtained for linear equations in finite differences, the solutions obtained from them by this process are free from all restriction. The solutions in series can be written down at once from the equation itself, inasmuch as each series has its own independent scale or law of relation; and no difficulties arise from the appearance of equal or imaginary roots in the equation determining the incipient terms of the series. These circumstances do indeed cause a certain variation of form; but they do not compel us to resort to any special process in each individual case.

The perfect separation and independence of the scales, or laws of relation of the series enables the author to discuss the characters of the series with reference to their convergency or divergency, and to

classify these equations into sets having peculiar and distinguishing properties in regard to this subject.

The first set includes those equations whose solutions can always be found in *convergent* series of *ascending* powers of the independent variable; and if in such case the equation be solved in series of *descending* powers (which can be done by this process), those series are certainly always *divergent*.

The distinguishing marks of this class of equations are,—that the factor of the highest differential coefficient contains one term only; and that (the terms being arranged in an ascending order) when this term is x^p , the factor of the next differential coefficient must not contain a term lower than x^{p-1} , the next not lower than x^{p-2} , and so on to the end.

The second set includes those equations whose solutions can always be found in *convergent* series of *descending* powers of the independent variable; and if in such case the equation be solved in series of *ascending* powers, they are always *divergent*.

The distinguishing marks of this class of equations are,—that the factor of the highest differential coefficient contains one term only; and that when this term is x^p , the next factor must stop at x^{p-1} , the next at x^{p-2} , and so on to the end.

The third set includes equations whose solutions can be found in series of *ascending* powers which for some values of the independent variable are *convergent*, and for other values *divergent*; and whose solutions can also be found in series of *descending* powers which are *divergent* for all values for which the other series are *convergent*, and *convergent* for all values for which the other series are *divergent*.

The distinguishing marks of this class of equations are,—that the factor of the highest differential coefficient contains two terms only, and that with reference to the first of such terms the equation is under the restriction mentioned with regard to the first set, and that with reference to the second of such terms it is under the restriction mentioned with regard to the second set.

The fourth set includes equations whose solutions are or may be *divergent* for some values of x , both in the ascending and descending series. In some cases, the ascending series is necessarily *divergent*, and the descending series convergent or divergent according to the value of x ; in other cases, the descending series is necessarily *divergent*, and the ascending series convergent or divergent according to value; and in the remaining cases, both series are convergent or divergent according to value, but not so as to be necessarily complementary to each other in this respect.

The distinguishing marks of this class are,—that the first factor may contain more than two terms; and that *either* the restriction of the first set is transgressed with reference to the highest term, *or* the restriction of the second set is transgressed with reference to the lowest term. In this set the divergency arising from value is of a finite character; and, as the series approach without limit to ordinary recurring series, there is a probability that the passage from convergency to divergency is not attended with danger.

The fifth set includes equations whose solutions, whether in ascending or descending series, are always *necessarily divergent*.

The distinguishing mark of this class is, that it transgresses *both* the restrictions to one or other of which the last set is subjected. In this case the divergency is infinite, and appears to be of an unmanageable character.

The analogy of the process leads to a presumption, that in all cases of divergency, above referred to, the corresponding convergent solutions are in series infinite in both the ascending and descending directions.

The author observes in conclusion, that the inverse calculus of the process here developed may be employed for the discovery of the generating functions of series whose laws of relation are given.

April 26.—“A Report upon further Observations of the Tides of the English Channel made by order of the Lords Commissioners of the Admiralty in 1848, with remarks upon the Laws by which the Tidal Streams of the English Channel and German Ocean appear to be governed.” By Captain F. W. Beechey, R.N., F.R.S. Communicated by the Lords Commissioners of the Admiralty.

The author commences this report by observing, that the result of the observations upon the tides in the English Channel, made in the course of the summer of 1848, had confirmed in a satisfactory manner the view he had taken of the tidal phenomena of the channel, in the report communicated to the Royal Society last year, and printed in the Philosophical Transactions (Part I. 1848), namely, that there is a meeting and a separation of the streams between Alderney and the Start: that the whole space between the Start and Scilly is under the joint influence of the channel and offing streams: that from the vicinity of the Start to the vicinity of Hastings the stream runs true up and down the channel; and moreover that this stream throughout turns nearly simultaneously with the time of high and low water on the shore at the virtual head of the tide, which he places in the vicinity of Dover; and lastly, that the streams which meet off the Start are turned down into the Gulf of St. Malo, and *vice versa*.

He then takes a comprehensive view of the tidal system of the English Channel and German Ocean together, and considering them as one great canal open at both extremities to the free admission of a great tidal wave, which might be supposed to meet and form a combined or stationary wave (art. 187, Encyclopedia Metropolitana), he infers that in such a case, there ought to be in the *eastern half* of such a canal, a recurrence of the phenomena which had been found to exist in the *western half*. He proceeds to explain that, from a valuable series of observations in the German Ocean by Captain Washington, R.N., and other authorities, it does appear that, inverting the direction of the stream, there is a correspondence of phenomena in almost every respect: that the offing and channel streams meet off Lynn, as off the Gulf of St. Malo, at the same hours, and at the same distance nearly from the virtual head of the tide: that the phase of the tide at Lynn corresponds with the phase of the

tide at Jersey : that there is an increased rise there also ; and that from the meeting of the tides off Lynn to the meeting of the streams off Dover, there is, as in the former case, a stream which turns nearly simultaneously with the high and low water on the shore at Dover ; the incoming and outgoing streams coinciding with the rising and falling water there ; and that there is, in fact, a complete identity of tidal phenomena in both parts of the supposed canal ; of this an illustration is given in two plans.

The author states that the meeting of the waves which enter the canal at opposite points does not occasion a stationary point of permanent slack-water, but one wave alternately prevails, so that the point of slack-water oscillates between Ramsgate and Hastings nearly, and occasions an inversion of the stream at about two hours before that of the true stream of the channel. He thinks it convenient for the purposes of navigation to consider this an *intermediate* stream, although in reality it is only a shifting of the place of the meeting and divergence of the opposite channel streams. To illustrate this part of the paper a table is given, in which the courses of the streams in various compartments of the supposed canal are given at every hour of the tide.

The author thinks this system of tides sufficiently established for the purposes of navigation, but with regard to the perfectly simultaneous motion of the stream throughout the stationary wave, he is of opinion that nothing but simultaneous observations will be considered satisfactory to science upon such a point, and these he hopes will be supplied by the observations of the ensuing summer.

The advantage of referring the motion of the stream to a standard such as that of the Dover tide-table will, it appears, be sensibly felt by the mariner, who will now have his course through the moving waters of the channel rendered simple and plain, instead of being perplexed with unsatisfactory references, and with calculations which in too many instances, it is believed, have caused the set of the tide to be wholly disregarded.

The want of a standard to which desultory observations, made in various parts of the channel, could be referred, the author believes to have been the occasion of several erroneous impressions of a somewhat dangerous tendency to navigation. As such he considers the following :—that the tide in all parts of the channel partakes of a rotatory motion and is never at rest, and that a ship's reckoning will never be far out in consequence, as she will never be carried far in one direction : that a vessel arriving off the Start at low water could, by sailing seven or eight knots an hour, carry ten or eleven hours favourable tide to Beachy Head : that in the German Ocean the stream sets north-east on one side, whilst it is running south-west on the other : that there is a tide and half-tide in the channel, so that when the stream has done in shore, by standing out, a ship will carry the stream three hours longer, or nine hours in one direction : and lastly, that the stream runs strongest at high and low water throughout the channel, and is motionless at half-tide.

These impressions do not appear to be justified by the observa-

tions. The stream when not diverted by rivers or estuaries appears to run true up and down the channel, and from side to side nearly; between the Start and Hastings, in the English Channel, scarcely varying a point for nearly five hours; and in the German Ocean for about four hours; the varying of the stream there being due, in the author's opinion, to the influence of the Thames and the rivers of Holland. As the stream turns nearly with the high and low water on the shore at Dover, there cannot be nine hours' current in one direction. With regard to the time at which the stream attains its greatest strength, he states that all the observations agree in fixing it at about half-tide (Dover).

The erroneous impressions above mentioned, the author considers have arisen from the times of the observations when made having been referred to the times of high water at places differing *two or three hours* from the time of high water at the *head of the wave*, or from an early popular opinion that the turn of the stream in the offing coincides with the rise and fall of the water on the shore.

The paper concludes with some remarks on the forms of the tide-wave between Cromarty and the Land's End, which are exhibited in two plans at every hour of the tide, obtained from a combination of the ranges and establishments of Dr. Whewell with those of M. Chazallon; and attention is particularly drawn to the relative lengths of the *stationary wave* and the *waves* by which it is generated; the *former* wave being only *half the dimensions of the latter*. These forms are exhibited on a reduced scale, but much exaggerated in height, and afford a comparison between the curve assumed by the stationary wave and that which the waves would have assumed had they rolled on in an uninterrupted course.

May 3.—“On the Reduction of the Thermometrical Observations made at the Apartments of the Royal Society from the year 1774 to 1781, and from the year 1787 to 1843.” By James Glaisher, Esq. of the Royal Observatory, Greenwich. Communicated by John Lee, Esq., LL.D., F.R.S. &c.

In this paper the author states that he has examined all the thermometrical observations which have been made at the Apartments of the Royal Society, with the view of ascertaining whether the diurnal variations at different epochs were in accordance with those which he had determined from the Greenwich observations, and which are contained in his paper published in the Philosophical Transactions for 1848. The result of this investigation was, that the corrections contained in the tables in his former paper were applicable to the observations of all the years since 1774.

The author is led from these examinations to the conclusion,—1st, that the instruments used have been uniformly good; 2ndly, that the observations have been faithfully recorded as read from the instruments; 3rdly, that the readings have been taken with great care with respect to the times stated; and lastly, that the observations were well worth the necessary labour of reduction. He finds, however, that some of the more recent observations of the self-registering instruments are liable to some uncertainty.

Having satisfied himself that the observations were well worth any amount of labour that might be bestowed on them, the author was anxious to reduce them to a useful form, but, in consequence of the great amount of work that would be required for the reduction of so extensive a series, he for some time hesitated to enter upon this labour. Finding however that there was a demand for the results of trustworthy observations extending backwards many years, and having, besides, the hope of connecting the Greenwich series of observations with these, he determined to perform the work. He states that the mean temperature of every month was determined in the first instance from the observations which had been made during the day, and secondly, from the observations of the self-registering instruments. Tables are appended to the paper showing the monthly, quarterly and yearly mean temperatures, with those of groups of years, and other tables exhibiting the departure of every individual result from the mean of all.

The author concludes by stating, that hitherto the mean temperature at Somerset House has been estimated a great deal too high. He does not here enter into the investigation as to whether the temperature as now determined is too high for the geographical position and elevation of Somerset House, but proposes to do so, in a paper he is preparing with the view of connecting the Somerset House with the Greenwich series, and of bringing up all the results to the present time. He hopes also, at some future time, to present results from the barometrical observations arranged in a similar manner.

May 24.—1. An appendix to a paper "On the Variations of the Acidity of the Urine in the State of Health"—"On the Influence of Medicines on the Acidity of the Urine." By Henry Bence Jones, M.D., M.A., F.R.S. &c.

The variations of the acidity of the urine in the state of health having been shown in the original paper, and the effect of dilute sulphuric acid also traced; in this appendix the influence of caustic potash, of tartaric acid, and of tartrate of soda, on the acidity of the urine is determined.

One ounce of liquor potassæ, specific gravity 1072, was taken in distilled water, in three days. It hindered the acidity of the urine from rising long after digestion to the height to which (from comparative experiments) it otherwise would have done; but it by no means made the urine constantly alkaline; nor did it hinder the variations produced by the state of the stomach from being very evident.

354 grains of dry and pure tartaric acid dissolved in water were taken in three days. The conclusion from the observations is that this quantity increased the acidity of the urine, but during that time it did not render the effect of the stomach on the reaction of the urine less apparent than when no acid was taken; and therefore, that this quantity of tartaric acid, during this time, does not produce so much effect on the reaction of the urine as the stomach does.

Tartrate of potash in large doses produces the most marked effect on the alkalescence of the urine. 120 grains of pure dry tartrate

of potash dissolved in four ounces of distilled water made the urine alkaline in thirty-five minutes. In two hours the alkalescence had disappeared, but after the next meal the effect of the tartrate of potash was again apparent. 10 drachms of tartrate of potash taken in three days produced but little, if any effect, on the acidity of the urine twenty-four hours after the last dose was taken.

2. "On the direct production of Heat by Magnetism." By W. R. Grove, Esq., M.A., V.P.R.S. &c.

The author recites the experiments of Messrs. Marrian, Beatson, Wertheim and De la Rive on the phenomenon made known some years ago, that soft iron when magnetized emitted a sound or musical note.

He also mentions an experiment of his own, published in January 1845, where a tube was filled with the liquid in which magnetic oxide had been prepared, and surrounded by a coil; this showed to a spectator looking through it a considerable increase of the transmitted light when the coil was electrized.

All these experiments the author considers go to prove that whenever magnetization takes place a change is produced in the molecular condition of the substances magnetized; and it occurred to him that if this be the case, a species of molecular friction might be expected to obtain, and by such molecular friction heat might be produced.

In proving the correctness of these conjectures difficulties presented themselves, the principal of which was that with electro-magnets the heat produced by the electrized coil surrounding them, might be expected to mask any heat developed by the magnetism. This interference, after several experiments, the author considers he entirely eliminated by surrounding the poles of an electro-magnet with cisterns of water, and by this means and by covering the keeper with flannel and other expedients, he was enabled to produce in a cylindrical soft-iron keeper when rapidly magnetized and demagnetized in opposite directions a rise of temperature several degrees beyond that which obtained in the electro-magnet, and which therefore could not have been due to conduction or radiation of heat from such magnet. A series of experiments is given with this apparatus.

By filling the cisterns with water colder than the electro-magnet, the latter could be cooled by the water while the keeper was being heated by the magnetization.

The author subsequently obtained distinct thermic effects in a bar of soft iron placed opposite to a rotating permanent steel magnet, using a delicate thermo-electrical apparatus placed at his disposal by Mr. Gassiot.

To eliminate the effects of magneto-electrical currents, the author then made similar experiments with non-magnetic metals and with silico-borate of lead, substituted for the iron keepers, but no thermic effects were developed.

He then tried the magnetic metals nickel and cobalt, and obtained thermic effects with both, and in proportion to their magnetic intensity.

Some questions of theory suggested by the above experiments and relating to the rationale of the action of what are termed 'the imponderables' and to terrestrial magnetism, are then briefly discussed, and the author concludes by stating that he considers his experiments prove satisfactorily, that whenever a bar of iron or other magnetic metal is magnetized, its temperature is raised.

XXI. *Intelligence and Miscellaneous Articles.*

ON THE PREPARATION OF PURE OXIDE OF COBALT.

BY M. LOUYET.

THE author observes that chemists are generally aware that nothing is less easy than to separate oxide of cobalt from the iron and nickel which it contains; on this account the oxide of cobalt is rarely met with in commerce perfectly free from all traces of these metals.

Among the methods which have been proposed to obtain from cobalt ores the oxide perfectly free from iron and nickel, M. Liebig's appears to have been preferred. This process, which depends chiefly on the property of sulphate of cobalt to resist a red heat, whilst the sulphates of iron and nickel are totally decomposed, may undoubtedly give good results, as applied to ores; but the author states that his researches in all other cases have failed to produce perfectly pure oxide of cobalt.

The author found that although sulphate of iron, when exposed alone to a red heat, is perfectly decomposed, yet when mixed with a large quantity of sulphate of cobalt, it sustained it without being decomposed, and consequently without being rendered insoluble; the heat was very great, and kept up for several hours. If a mixture containing cobalt, but no arsenic, be treated according to M. Liebig's process, the iron, not being converted into an insoluble arseniate, remains in the mass partly in the state of sulphate. This takes place with zaffre, an arsenical ore of cobalt which has been roasted, and from which the arsenic has consequently been expelled. It results from what is above stated, that the sulphate of iron formed, partially resists the action of the heat, and consequently that the cobalt eventually obtained contains iron, though completely free from nickel.

Although processes are already known by which cobalt and iron are separated without much difficulty, the author states that one which he has discovered is so simple, and its employment so certain, that he thinks in making it known he may render service to the arts which include the use of pure oxide of cobalt.

This process depends on the difference which exists between the affinities of oxide of cobalt on one hand, and on the other of the protoxide and sesquioxide of iron for acids; a difference which is highly advantageous to the first of these compounds. Take a solution of sulphate of cobalt, containing a certain quantity of sulphate of iron, and add to this gelatinous hydrate of cobalt at least equivalent to the oxide of iron contained in the sulphate; heat the mixture

to ebullition for some time; the violet colour of the hydrate of cobalt will soon disappear, and it will be replaced by a precipitate of a dirty yellow colour. In this case the hydrate of cobalt decomposes the sulphate of iron, is substituted for the proto- or sesquioxide of iron, which it renders insoluble, and precipitates in the state of hydrated sesquioxide; this hydrate is necessarily mixed with a small quantity of hydrate of cobalt, which should be used in excess, in order to be certain of the complete precipitation of the oxide of iron. Nothing remains to be done but to filter and wash the precipitate. The salt of cobalt thus obtained yields no indication of the presence of iron to the most sensible reagents. It yields, accordingly, no precipitate whatever when, after having added hydrochlorate of ammonia to it, it is supersaturated with great excess of ammonia.

For the sake of greater simplicity, a solution of carbonate of soda may be employed instead of the hydrate of cobalt, and the mixture digested at a boiling heat for some time; in both cases the precipitate will contain all the iron. [This appears to be identical with the method proposed by Scheerer for separating iron from cobalt, described in the *Phil. Mag.* for February 1840.—W. F.]

To recapitulate: it follows that in order to purify oxide of cobalt it is sufficient to dissolve it in dilute sulphuric acid, to evaporate to dryness, and calcine at a red heat, treating the mass with boiling water, and adding to the solution either hydrate of cobalt or carbonate of soda in the manner described. Sulphate of nickel appears to decompose more readily than sulphate of iron, under the circumstances described; when zaffre is treated according to M. Liebig's process, it yields oxide of cobalt quite free from nickel, but contains a large quantity of oxide of iron.—*L'Institut*, Juin 27, 1849.

ON ALUMINATE OF COBALT. BY M. LOUYET.

M. Louyet remarks it as well known, that alumina and the salts of cobalt may combine in certain circumstances, and form a fine blue colour. It is by this process that M. Thenard prepared the blue which bears his name, by mixing certain proportions of alumina and phosphate or arseniate of cobalt, and subjecting the mixture to a long-continued calcination. All these substances are employed in the gelatinous or hydrated state. As the precipitate formed by carbonate of soda in a mixture of alum and a salt of cobalt also yields a blue compound by calcination, it has been inferred that the constituent elements of cobalt-blue were alumina and oxide of cobalt, so combined that the compound may be considered as a salt, or an aluminate of cobalt.

The facts which M. Louyet has ascertained are, that the substance obtained by the mixture of gelatinous alumina and phosphate or arseniate of cobalt, also in the gelatinous state, yields a blue when subjected to a red heat. At this temperature the mixtures of gelatinous alumina and hydrated oxide of cobalt yields only *blacks* or *grays*, whatever may be the proportions of the constituents. In order that the alumina and cobalt may produce blue, the mixture must be exposed to a temperature very near that of melting glass. It results

from this that the presence of certain fixed acids is favourable to the combination of alumina with oxide of cobalt. These observations explain the cause of failure which chemists occasionally incur, in attempting to prepare Thenard's blue without employing phosphate or arseniate of cobalt.—*L'Institut*, Juin 27, 1849.

DETECTION OF IODINE AND BROMINE.

BY M. ALVARO REYNOSO.

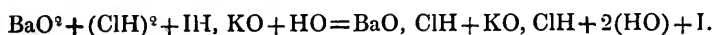
The method employed to ascertain the presence of these bodies, when they exist as iodides or bromides, the author remarks, consists in dissolving them in water, to add starch in the state of paste, or æther and a few drops of solution of chlorine. The chlorine seizes the metal combined with the iodine or bromine, and these bodies colour the starch blue, or dissolve in the æther; but iodine and bromine having the property of combining directly with chlorine, and of forming a chloride of iodine or of bromine, the chlorine, in order to detect the presence of these bodies, ought not to be employed in excess; because the chlorides of iodine or bromine are decomposed by contact with water and produce hydrochloric acid, and iodic or bromic acid without acting on the starch or the æther.

This experiment was very difficult to perform; often, indeed, these bodies could not be discovered, and this was supposed to be owing to the above-described difficulty. Then the quantity of chlorine was diminished, from the fear of exceeding the requisite proportions, and it happened that the quantity of chlorine was not sufficient to set the iodine at liberty. The manner in which the chlorine was employed also increased this error; in fact, it is well known that a solution of chlorine is weakened by keeping, and that eventually its power is lost, in spite of every possible precaution. Thus on pouring into a solution of an iodide or bromide a very small quantity of aqueous solution of chlorine, it happened that the iodine was not set free, and that all the chlorine was employed in forming hydrochloric acid. This method, then, was not applicable to the detection of small quantities of iodine or bromine, especially when these bodies are mixed with substances capable of seizing the chlorine. It was therefore desirable that the iodine or bromine should be isolated by means of a body incapable of acting upon them, whatever might be its excess. Oxygenated water fulfills these conditions; it decomposes hydriodic or hydrobromic acid without at all acting upon the iodine or bromine set free by it.

The following is the method of proceeding for iodine: a bit of binocide of barium is to be put into a small glass tube closed at one end; then are to be added to it distilled water, pure hydrochloric acid, and paste of starch; the operator is to wait till bubbles arise to the surface, and then the iodide is to be added. A rose-blue colour is immediately procured if the quantity of iodine is but small, but of a deep blue if the quantity of iodine is considerable.

It is more convenient to operate on these conditions; not only considering them as manipulations which become very easy, but also with regard to the success of the experiment. On this plan, the

requisite excess of oxygenated water is certain to be employed when hyposulphites, sulphates, or sulphurets are present; besides, the hydrochloric acid employed in the preparation of the oxygenated water, acts an important part, for it serves to set hydriodic acid free,



Although it is unquestionable that the hydrochloric acid, by reacting on the binoxide of barium, in the presence of water, produces oxygenated water, the author was desirous of satisfying himself that it was in fact HO^2 which actually produced the result obtained; for this purpose he substituted tartaric for hydrochloric acid, and obtained the same result. M. Thenard had also described the decomposition of hydriodic acid by pure oxygenated water.

When the iodides are mixed with chlorides, sulphurets, sulphites or hyposulphites, the process is equally correct; only, as by the action of hydrochloric acid on the sulphuret sulphuretted hydrogen is produced, which is decomposed by oxygenated water, and the hyposulphites and sulphites are converted into sulphate by absorbing oxygen, a larger quantity of oxygenated water is required than if the iodide was pure.

The hyposulphites and sulphites, on becoming sulphates, produce a precipitate of sulphate of barytes in the liquor; this might delay the action, if agitation were not employed to detach the sulphate of barytes from the surface of the binoxide of barium: it is also a precaution which ought always to be adopted to increase the production of oxygenated water. By this process the presence of iodine is readily detected in the urine of a patient taking 0.10 centigr. of iodide of mercury morning and evening. In the same urine no iodine could be detected by means of chlorine; this is therefore a case in which, notwithstanding every precaution, the iodine passed undetected by chlorine.

This process detects the presence of iodine in the ash of sponge. A drop of a solution of 0.010 grm. of iodide of potassium dissolved in a litre of water, produced, each time that it fell into the tube, a manifest blue colour at the surface. By agitation the blue colour disappeared, and the liquor assumed a rose tint; on adding another drop, a fresh blue colour is obtained at the surface. This process, therefore, very easily indicates less than $\frac{1}{100,000}$ of iodide of potassium. The process is the same for bromine, excepting that instead of starch æther is employed; agitation is used, the bromine dissolves in the æther, and it becomes of a more or less deep yellow colour according to the quantity.

When, however, iodides and bromides occur mixed, they are detected by adding an excess of starch and of æther. The iodine combines with the starch, and the bromine, dissolving in the æther, rises to the surface; so that the blue colour is obtained below, and the yellow tint high up.—*Comptes Rendus*, Avril 30, 1849.

ON THE CHEMICAL NATURE OF THE EGG.

M. Barreswil has presented to the Academy of Sciences a memoir in which he states that he has found sugar in the albumen of the egg, and that the albumen is alkaline owing to the presence of carbonate of soda; he finds also that the yolk contains little or no alkali, and that its emulsive property is derived from a product resembling the pancreatic juice, which is not acid, and becomes so only by undergoing alteration. He further states that the acid reaction and properties of the gastric juice are owing to organic acid, and not to hydrochloric acid.—*L'Institut*, Juin 20, 1849.

ON THE FORMATION OF FATTY MATTERS IN VEGETABLES.

M. Blondeau de Carolles remarks it as well known, that oleaginous grains do not, in an early stage of their development, contain any trace of fatty substances; and that it appears evident that the latter are formed from the substances originally entering into the constitution of these grains. The author was desirous of determining which are the principles, the transformation of which gives rise to the fatty bodies, and the mode in which it is effected.

The author especially studied the formation of oil in the olive. This fruit, carefully analysed, yielded scarcely an appreciable trace of nitrogen; it was not therefore owing to the presence of vegetable albumen or caseine, neutral nitrogenous bodies contained in oleaginous grains, the transformation of which, according to MM. Liebig and Dumas, is sufficient to explain the formation of fatty substances.

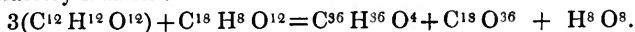
The elements of this transformation must then be sought for in a body analogous in composition to fecula, sugar or lignin.

The analyses of olives, at first performed in an early state, and afterwards as they approached maturity, showed that in the former condition they contained no trace of oil; but that from the moment at which the oil began to appear till the fruit had attained its complete development, the proportion of tannin, and especially of lignin gradually diminished as that of the oily liquid increased.

From these circumstances the author concludes, that the formation of oil in the olive is the result of the reciprocal action of tannin and lignin, and experiment seemed to confirm this explanation: some olives confined in a graduated receiver full of mercury disengaged after a few days, pure carbonic acid gas, and the sides of the receiver were covered with the [condensed?] vapour of water.

The carbonic acid was four times greater in volume than that of the olives from which it was produced; and it could be formed only at the expense of the elements contained in the fruit, the external air being excluded.

The following formulæ explain the phænomena described in a satisfactory manner:—



Lignin. Tannin. Oil. Carbonic acid. Water.

This explanation was verified by experiment.

The analyses of the oil extracted from olives which had served for experiment gave the following results :—

Oil 0·361.	Carbonic acid 1·043.	Water 0·377.
Carbon 78·66	According to the formula $C^{36}H^{36}O^4$ there should be obtained	C 76·05
Hydrogen . . 11·39		H 12·95
Oxygen 9·95		O 11·00
100·00		100·00

These numbers approach each other sufficiently near to admit the formula as expressing one of the most curious transformations presented by organic chemistry.—*L'Instiut, Juin 20, 1849.*

METEOROLOGICAL OBSERVATIONS FOR JUNE 1849.

Chiswick.—June 1, 2. Fine. 3. Fine: hazy: clear. 4. Very fine. 5. Sultry: showery at night. 6. Uniformly overcast. 7. Fine. 8. Fine: overcast. 9. Fine: rather cold: overcast: clear. 10—12. Overcast. 13. Fine: cloudy: clear. 14. Fine: slightly clouded. 15. Dusky haze. 16. Overcast: fine. 17. Fine: dusky haze. 18. Fine. 19. Slight rain. 20. Very fine. 21. Fine. 22, 23. Very fine. 24. Cloudless and very fine. 25. Overcast: very fine. 26—28. Very fine. 29. Cloudy: rain. 30. Cloudy: clear and cold at night.

Mean temperature of the month	59°·30
Mean temperature of June 1848	59·58
Mean temperature of June for the last twenty-three years ...	60·85
Average amount of rain in June	1·88 inch.

Boston.—June 1. Cloudy. 2—4. Fine. 5. Cloudy: rain, with thunder and lightning A.M.: rain P.M. 6—9. Fine. 10. Cloudy: rain early A.M. 11. Cloudy. 12. Cloudy: rain P.M. 13, 14. Fine. 15. Fine: rain P.M. 16. Cloudy. 17, 18. Fine. 19. Fine: rain A.M. and P.M. 20. Fine. 21. Cloudy. 22. Fine. 23. Cloudy. 24. Fine. 25. Rain. 26. Fine. 27. Cloudy. 28. Fine. 29. Fine: rain P.M. 30. Cloudy.

Applegarth Manse, Dumfries-shire.—June 1. Clear and bracing weather: shower A.M. 2. Fine and growing: one shower P.M. 3, 4. Fair and fine. 5. Fair A.M.: one shower P.M.: electr. 6, 7. Fair and fine. 8. Fair, but chilly from N.E. 9. Fair: air highly electric: thunder. 10. Fair and very droughty. 11. Fair and droughty: getting cloudy. 12. Fair and droughty: cleared away. 13. Fair and droughty. 14. Fair and droughty, but getting cloudy. 15. Fair and droughty: cloudy: thunder. 16. Slight shower: much thunder. 17. Again droughty. 18. Cloudy: a few drops of rain. 19. Heavy rain, night: shower, day. 20. Frosty during night: shower P.M. 21. A few drops: very high wind. 22. Rain at intervals. 23. Fair and clear. 24. Light rain: very mild. 25. Cloudy A.M.: slightly showery. 26. Slight shower P.M. 27. Fine: warm: fair all day. 28. Fair. 29. Rain P.M., not heavy but soft. 30. Fair all day and warm.

Mean temperature of the month	53°·3
Mean temperature of June 1848	55·7
Mean temperature of June for twenty-five years	56·1
Rain in June for twenty years	3·16 inches.

Sandwich Manse, Orkney.—June 1, 2. Bright: clear. 3. Bright: cloudy. 4. Showers: cloudy. 5, 6. Bright: clear. 7. Hazy: clear. 8. Bright: clear. 9. Bright: snow-showers. 10. Cloudy. 11. Bright: clear. 12. Bright: cloudy. 13, 14. Bright: clear. 15. Cloudy. 16. Drizzle: rain. 17. Clear: rain. 18. Rain. 19. Cloudy: damp. 20. Cloudy: rain. 21. Rain: clear. 22. Cloudy. 23. Rain: cloudy. 24. Bright: clear. 25. Bright: rain. 26. Fog: drizzle. 27. Showers: hazy. 28, 29. Bright: clear. 30. Bright: rain.

Days of Month.	Barometer.				Thermometer.				Wind.				Rain.			
	Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.	
	Max.	Min.	8 $\frac{1}{2}$ a.m.	9 a.m.	9 $\frac{1}{2}$ a.m.	8 $\frac{1}{2}$ p.m.	Max.	Min.	8 $\frac{1}{2}$ a.m.	Max.	Min.	8 $\frac{1}{2}$ a.m.	8 $\frac{1}{2}$ p.m.	Chiswick.	Dumfries-shire.	Orkney, Sandwick.
1849. June.																
1.	30.088	30.040	29.45	29.83	29.50	29.55	78	48	63	60	47	56	51	sw.	ws.	s.
2.	30.162	30.076	29.55	29.97	29.60	29.66	75	47	71	62	50	52	49	w.	calm	sw.
3.	30.222	30.117	29.74	30.11	29.70	29.85	75	51	71	65	41 $\frac{1}{2}$	53	49	e.	calm	sw.
4.	30.101	29.939	29.63	30.05	29.77	29.77	81	57	68	69 $\frac{1}{2}$	41 $\frac{1}{2}$	57	52	w.	calm	sw.
5.	29.987	29.795	29.35	29.84	29.78	29.81	85	57	66	68	49	51	49	w.	e.	wnw.
6.	30.109	30.090	29.62	30.05	29.75	29.81	66	51	63	62 $\frac{1}{2}$	43	54	54	ne.	calm	w.
7.	30.237	29.920	29.78	30.15	29.72	29.77	71	50	60	66 $\frac{1}{2}$	38	51	47	nw.	calm	e.
8.	29.990	29.892	29.56	30.00	29.79	29.73	66	42	65	58	38	48	46	ne.	calm	nw.
9.	29.918	29.793	29.50	29.90	29.62	29.44	65	43	55	60	37	47	39	ne.	calm	n.
10.	29.728	29.668	29.30	29.71	29.52	29.58	60	42	46	58	38	49	44	ne.	calm	n.
11.	29.851	29.797	29.13	29.82	29.50	29.48	65	37	55	59	35	43	43	ne.	nw	w.
12.	29.933	29.923	29.50	29.81	29.48	29.61	57	40	53	58	42	47	44	nw.	n.	nw.
13.	30.118	29.927	29.55	30.00	29.67	29.69	67	34	56	61	39	47	45	ne.	w.-s.	w.
14.	30.175	30.116	29.67	30.02	29.71	29.74	72	40	59	59	36	49	44	ne.	w.	w.
15.	29.899	29.791	29.52	29.90	29.75	29.76	70	48	63	55	33	49	48	e.	s.-se.	e.
16.	29.778	29.750	29.35	29.84	29.64	29.63	68	46	61	64	42	51	47	n.	calm	ne-sw
17.	29.975	29.924	29.43	29.89	29.63	29.54	72	40	63	62 $\frac{1}{2}$	43	49 $\frac{1}{2}$	48	nw.	calm	sw.
18.	30.066	30.038	29.59	29.92	29.78	29.72	75	41	62	60	43	53	48	w.	calm	w.
19.	29.906	29.822	29.33	29.48	29.66	29.60	60	43	58	60	50	48	46	sw.	s.	nw.
20.	30.139	30.086	29.64	29.98	29.92	29.74	75	50	58	59	35 $\frac{1}{2}$	50	47	w.	calm	sw.
21.	30.118	30.050	29.50	29.83	29.56	29.76	75	42	63	55	50	52	47	sw.	calm	n.
22.	30.166	30.011	29.64	30.00	29.78	29.96	78	48	62	59	48 $\frac{1}{2}$	51	47	w.	w.	ese.
23.	29.941	29.822	29.38	29.73	29.61	29.72	85	52	68	62	50	52	48	sw.	w.	sw-w.
24.	29.904	29.880	29.40	29.79	29.85	29.92	89	46	63	60	51	51 $\frac{1}{2}$	49	ne.	e.-s.	n.
25.	29.979	29.907	29.50	29.81	29.72	29.88	79	53	58	64	47	54	51	se.	wnw.	se.
26.	29.980	29.919	29.37	29.77	29.78	29.74	75	51	65	60	52	54	53	w.	wnw.	s.
27.	29.969	29.943	29.40	29.76	29.64	29.68	78	47	66	59	51 $\frac{1}{2}$	54	46	sw.	w.	sw.
28.	30.128	29.992	29.44	29.89	29.92	30.04	76	48	61	64	48	50	47	nw.	w.	nw.
29.	30.079	29.919	29.62	29.95	30.01	30.00	73	55	60	59	46	50 $\frac{1}{2}$	47 $\frac{1}{2}$	s.	e.	wnw.
30.	30.156	29.916	29.50	30.02	30.07	29.90	63	35	56	64 $\frac{1}{2}$	44 $\frac{1}{2}$	52	49 $\frac{1}{2}$	ne.	nne.	e.-w.
Mean.	30.027	29.929	29.49	29.894	29.724	29.745	72.47	46.13	61.5	61.7	43.8	51.21	47.63	0.31	2.46	0.72 2.33

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[THIRD SERIES.]

SEPTEMBER 1849.

XXII. *On the Production of Lightning by Rain.*
By WILLIAM RADCLIFF BIRT*.

ON the 26th of July, 1849, a severe thunder-storm, accompanied by the destruction of property and the loss of human life, passed over the metropolis. About 1^h 30^m P.M. the clouds towards the north-east presented a very dark and threatening aspect; they assumed an inky colour, and the velocity of their motion was very slow; in fact the appearance noticed was strikingly of that character which the writer has frequently observed to precede a thunder-storm. On this occasion his attention was more particularly directed to the connexion between the electric discharge and the sudden gush of rain that more or less accompanies it, with a view to illustrate the question occurring in the Report of the Committee of Physics approved by the President and Council of the Royal Society, p. 46, "Is this rain a *cause* or *consequence* of the electric discharge?"† On the previous day, the 25th, about 3^h 50^m P.M., during a thunder-storm a sudden gush of heavy rain occurred,

* Communicated by the Author.

† The paragraph runs thus: "There is one point to which we wish that some attention might be paid,—it is the sudden gush of rain which is almost sure to succeed a violent detonation immediately overhead. Is this rain a *cause* or *consequence* of the electric discharge? Opinion would seem to lean to the latter side, or rather, we are not aware that the former has been maintained or even suggested. Yet it is very defensible. In the sudden agglomeration of many minute and feebly electrified globules into one rain-drop, the quantity of electricity is increased in a greater proportion than the surface over which (according to the laws of electric distribution) it is spread. Its tension therefore is increased, and may attain the point when it is capable of separating from the *drop* to seek the surface of the *cloud*, or of the newly-formed descending body of rain, which under such circumstances, and with respect to electricity of such a tension, may be regarded as a conducting medium. Arrived at this surface, the tension for the same reason becomes enormous, and a flash escapes."

Phil. Mag. S. 3. Vol. 35. No. 235. Sept. 1849.

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which within two seconds, by estimation, was *succeeded* by a vivid flash of lightning: the thunder occurred at a further interval of some few seconds. From this it would appear, provided the heavy rain fell over the entire space between the place of observation and that of the electric discharge, that it was not a *consequence* of that discharge, the gush occurring at a sensible interval previous thereto. The setting in of the storm of the 26th, about 1^h 45^m P.M., again called the attention of the writer to this point; and several flashes occurred between 1^h 45^m P.M. and 2^h 3^m P.M. without being preceded by a gush, although heavy rain more or less accompanied the discharges, in one or two instances almost simultaneously or immediately afterwards. At length, about 2^h 4^m P.M., a most violent and very remarkable gush of rain occurred, which was followed within one minute by a most vivid flash of lightning, the thunder succeeding almost instantaneously. The windows of the house in which the writer was observing the storm were sensibly shaken, and portions of the mortar between the arches over the windows and the frames thrust out, of course by concussion. Within a minute or two after this discharge a partial cessation of the heavy rain took place, but sudden gushes occurred at short intervals within the next six or seven minutes; they were, however, unaccompanied by lightning. At the end of this period the atmosphere presented a very remarkable appearance; a perfect stillness characterized the air, which possessed great transparency, so that the surrounding objects were seen with very minute distinctness of detail. The stillness and transparency during the time they continued, riveted the observer to their contemplation. It appeared that during this time the storm was hushed, and a calm of a rather extraordinary character succeeded it, during which a rather large break in the clouds was seen towards the south-east, and the entire phænomena at this time induced the idea that the weather was clearing up. Within ten minutes, however, the storm again burst forth; the lightnings played, the thunder roared, and heavy rain, mingled with hail so thickly that comparatively near objects could scarcely be distinguished, fell in torrents, and the writer observed during the remainder of the storm four or five sudden gushes that were quickly *succeeded* by lightning. On all these occasions he is quite certain that the sudden gush of rain *preceded* the electric discharge. Of all the discharges, which were very frequent, that at or near 2^h 5^m P.M. appeared to the writer to be *nearest* his own locality; the lightning appeared to him to be quite as, if not more, vivid than that at any other discharge; and the interval between it and the thunder was certainly the shortest, the thunder being

the loudest. Between 2^h 0^m P.M. and 2^h 15^m P.M., according to the concurrent testimony of several witnesses, and preceding the precipitation of the hail nearly half an hour, a discharge struck half-a-dozen houses, being Nos. 17 to 22 in West Street, North Street, Whitechapel, between one-fourth and one-third of a mile south of the writer's residence. The stroke appears to have presented some phænomena of an interesting and remarkable character. Immediately behind and to the west of the houses in question is situated a large open space of ground, known as a Jews' burying-ground, in which is a piece of water not far from the houses. The gable end of No. 22 faces the west, the fronts of the houses the north, and their backs the south; the flues of No. 22 form part of the wall at the gable end. At the front and between every alternate house is a metallic spout, for the purpose of carrying off the water from the roof; the water is received from the tiles in a leaden or zinc gutter, which is in metallic communication with the spout; but it does not appear that there is a metallic communication between the gutters from house to house: these metallic spouts are short, not in any case descending so low as the doors; they are replaced by wooden spouts, which convey the water to the street. It appears that the stroke perforated one of the chimney-pots of No. 22, descending the flue to the roof, which it stripped of a great portion of the tiles*, and then passed to the metallic spout in front, disrupting and tearing away the lower wooden spout, a piece of which, seven feet in length, it rent off and hurled with great violence into the back yard of one of the opposite houses which abutted on the north side of West Street: a great portion of the piece rent from the spout was shivered into small splinters about two inches long. A portion of the stream appears to have been conducted by the gutter communicating with the metallic spout of No. 22 to that between Nos. 20 and 21, down which it proceeded, thrusting the lower wooden spout about three or four inches from the wall, and chipping away portions of the brickwork in its passage through the front wall of No. 21, which it perforated (the aperture being of a considerable size), and immediately passed to the principal wheel of a silk-winding machine. A woman who was attending the machine, and had in her hand at the moment a spindle, received a severe shock, and was hurled by the force across the room. This stream appears to have given off a lateral stroke, which manifested its effects at the box of the lock of the street door, tearing away the plastering and paper as it passed into the front room, in

* The western wall of this house has been so shaken by the stroke that it is considered necessary that it should be taken down and rebuilt.

which a pane of glass was shattered. A woman who was near this room at the time described the passage as being filled with a light of a blue colour, and saw the lightning pass out of the back window over her child's head. It would appear that at the time the lightning was passing from one metallic conductor to another in the immediate front of the houses, a young man named Thomas Johnson opened the street-door of No. 17: he was accompanied by a lad of about fourteen years of age, who was, almost immediately after the door was opened, thrown backwards. The shock was so sudden that he knew not what happened to Johnson, who was found shortly after lying on his side, his back to the wall, and feet just beyond the threshold of the door, quite dead. He was evidently struck by the lightning, which, playing in the front of the houses, was probably attracted by the metallic spout that terminated a few feet above the door. The plaster just above the door, and which was most probably over his head, was torn away; but there is no mark or perforation to trace its entrance from above; the probability appears to be, that a stream struck upwards immediately after striking him. The two houses immediately opposite to Nos. 21 and 22 also received some injury from the lightning glancing sideways, as it were, from the principal stream which shattered the roof of No. 22.

It cannot fail to be remarked, that the principal mischief after No. 22 was struck occurred in the front, and more or less in the neighbourhood of the metallic spouts, which answered the purpose of *imperfect* conductors, the metallic continuity being *interrupted* on the roofs by the leaden gutters extending only a certain distance on each side of the metallic spouts, and also in front of the houses by the wooden spouts carrying the water to the earth. In connexion with the presence of the lightning in the front, the altitude of the houses struck renders it rather improbable that the stroke descended *immediately* from the cloud. The houses in West Street are the *lowest* in the neighbourhood; those in the street immediately to the north are rather higher; and at the further end of the street, near No. 1, are three houses in North Street considerably *higher*; so that had the stroke descended immediately from the cloud above, which it is to be presumed was at the usual elevation of clouds under these circumstances, the probability is that the *high* houses in North Street would have been struck rather than the *low* houses at the further end of West Street. Should it have been the case that the stroke emanated from an altitude but little above the houses struck, we have evidently to seek the cause of the discharge *below the*

cloud. It is extremely difficult to connect the stroke with the discharge at 2^h 5^m P.M.; although the concurrent testimony of the witnesses would lead to the high probability that such was the case, especially as one stated that it occurred some considerable time previous to the hail that fell. The general impression in the neighbourhood appears to have been, that the lightning shot or glanced over the houses to the north of West Street, when it struck the corner, and did not descend perpendicularly from a considerable height. The discharge at 2^h 5^m P.M. was, as we have already observed, *preceded* by a gush of heavy rain; and taking the suggestion in the report alluded to above into consideration, there appears to be great probability that the formation of the lightning was in accordance therewith. For upwards of half an hour violent meteorological action had taken place, the precipitation of rain being very prominent. There can be no question that this precipitation was accompanied by well-marked electrical phenomena; and when, as at 2^h 5^m P.M., an increased but sudden precipitation occurred, it is likely that an agglomeration of the smaller drops took place, increasing, as suggested, the electric tension to such an enormous extent, that a flash escaped *in the immediate neighbourhood of the houses struck*; and when we consider that at the time several millions of drops must have been falling, each contributing its quota to the general result, it is not to be wondered at that the tension of the electricity was so great as to produce the very violent effects witnessed.

Bethnal Green, August 4, 1849.

XXIII. *On Anharmonic Ratio*. By Professor DE MORGAN*.

GEOMETRICAL communications to scientific journals are not so common as they used to be, which may partly be attributed to the expense of woodcuts, and partly to the decline of taste for geometry. The latter is accelerated by the paucity of geometrical reading arising from the former cause: and its effect upon soundness of mathematical habits is deadly. But there is no reason why the expense of woodcuts should place any obstacle in the way. I am satisfied, from sufficient trial, that when proper description of the diagram is given in the text, the person who draws his own diagram from the text will arrive at the author's meaning in half the time which is employed by another, to whom the successive appearance of the parts is prevented by his seeing the whole from the be-

* Communicated by the Author.

ginning. The great work of M. Chasles has not a single diagram.

It will be asked whether the omission of diagrams will not cause a misprint (such as must sometimes occur) to be fatal to the reader's chance of arriving at the result. Putting out of view the very great confusion which a misprint causes when it shows a variance with the diagram, and leaves the reader unable to decide which is wrong, I will remark that it is possible very greatly to diminish the risk of error in the description of the diagram. This is all that is wanted: for grant the reader once able to establish the diagram, and his position is then as good as (at least, I say better than) that of the person who has had the use of a woodcut. My plan would be *to double every letter the first time it occurs*. Instead of 'Let AB be a given straight line, and C a point without it,' I should write 'Let AABBB be a given straight line, and CCC a point without it.' This would present, I think, a twofold advantage. First, neither author nor compositor is so likely to put in DD instead of BB as D instead of B. Secondly, in looking back to recover the meaning of a letter, the eye would be caught by the reduplication which marks its first appearance, whereas at present search is often necessary. I need not state that the omission of the diagram would compel writers to be complete: at present they sometimes (but always implying profession of the contrary) rely upon it that the reader will supply omissions by the diagram.

I propose in this present paper (which, however, will contain nothing elaborate enough to require any use of the preceding suggestion) to state the mode in which the theories of *transversals*, of the *anharmonic ratio*, and of the *complete quadrilateral*, in their simplest parts, may be connected together, by the help of a theorem which I believe to be new. I shall hardly state more than results. I use the geometrical language of composition of ratios, and symbols representative of it. It must also be noticed that no demonstration need involve any part of the sixth book except the first proposition.

(1.) Let $A : B$ represent the ratio of A to B, and $(A : B)(C : D)$ the ratio compounded of the ratios of A to B and C to D. The symbol $1 : 1$ may be used for a ratio of equality. (2.) When a point is stated to be on a line, let it be understood that it may be on the line produced; and when two lines are stated as meeting, the point of intersection may be in either or both produced. (3.) Two lines are said to be *internal* segments of their sum, *external* segments of their difference: so that AC, CB are always segments of AB, when A, B, C are in one line. (4.) When there are four points, P, Q, R, S,

in one line, in any order, let (PQRS) denote the ratio compounded of $PQ:QR$ and $RS:SP$, or $(PQ:QR)(RS:SP)$. This is what M. Chasles has styled the *anharmonic ratio*, by an extension of its well-known case in which (PQRS) is $1:1$, in which case PR is said to be harmonically* divided in Q and S. Since the letters P, Q, R, S can be written in twenty-four different orders, there are as many forms in which an anharmonic ratio may be presented. But all these forms are only three ratios and their inverses: nor are these three distinct, for they are always of the connected forms $A:B$, $A:A+B$, $B:A+B$. And the rules for separation and collection are as follows.

To find the distinct anharmonic ratios. Take any one, say (QSRP), advance the first letter one interval, and two intervals, giving (SQRP), (SRQP): these three are distinct; and if the first be $A:B$, the second is $A:C$, the third is $B:C$, and of A, B, C, some one is equal to the sum of the other two.

To find forms of equivalent ratio. Invert all the letters, or pairs, or one after the other; thus (RSPQ), (QPSR), (PQRS), (SRQP), are the same.

To find the forms which have ratios inverse to that in any given form. Change either extreme into the other, or interchange the first and third, or second and fourth: thus if (SQRP) = $A:B$, either of these four (QRPS), (PSQR), (RQSP), (SPRQ), is $B:A$.

To express the ratios circuitously, or in the order $A:B, B:C, C:A$. Make two transferences of the first letter and of the second: thus if (PQRS) be $A:B$, (QRPS) is $B:C$, and (PRSQ) is $C:A$.

To find convenient values of A, B, C. Look for the order in which the letters occur on the line, say P, Q, R, S. Take the rectangle under PQ and RS, with wholly unconnected sides; under PS and QR, one of which contains the other; and under PR and QS, in which each side is partly in and partly out of the other. Call these A, B, C; then $C=A+B$, and (PQRS) = $A:B$. Call this, and its inverse, the *primary ratio*.

(5.) If two systems be anharmonically equivalent, the six ratios (counting inverses) of one being severally equal to those of the other, then the primary ratios are equal, each to each.

(6.) To what is usually said on harmonic division, I should add the following. If C, D, and C', D' be conjugate pairs of points to AB, it is known that of the two lines CD, C'D', the

* This mode of speaking is not exactly consistent with the derivation of the phrase; it is PR which is an harmonic mean between PQ and QS, according to the received use of the word *harmonic*.

less is entirely within, or entirely without, the greater. Hence, P being any point, CPD and C'PD' cannot both be right angles; from which, by a very easy *reductio ad absurdum*, it follows that if CPD be a right angle, PC bisects the angle APB. For an easy* mode of finding any number of pairs of conjugates, with a given line AB, proceed as follows. Take BAP isosceles at A, and bisect BP in Q. Having a point C in AB, take in AP AC' = AC, and draw C'Q meeting AB in D: then C and D are conjugate.

(7.) If two triangles, ABC, A'B'C', have their bases, AB A'B', in the same line, which cuts CC' in Z, the areas are in the ratio compounded of the bases AB, A'B', and the segments of the line joining the vertices, CZ, C'Z. Join BC': then CAB: C'A'B' is (CAB: CZB)(CZB: ZBC')(ZBC': C'A'B'), or (AB: ZB)(ZC: ZC')(ZB: A'B'), or (AB: A'B')(CZ: C'Z). If the bases be equal, the areas are as CZ: C'Z. Hence the fundamental proposition on transversals is most easily proved. If the sides of ABC be cut by a line cutting BC in A', CA in B', AB in C', then the number of *external* sections is odd, and (AC': C'B)(BA': A'C)(CB': B'A) = 1:1. Join AA', CC' by vi. 1, and (7.): the compound just mentioned is (AA'C': BA'C') (BA'C': CA'C') (CA'C': AA'C') or 1:1. Many writers prove the converse incorrectly, both in this proposition and others. If there be A' B' C' on the three sides, with an odd number external, and (AC': C'B)(BA': A'C)(CB': B'A) = 1:1, then A', B', C' are in one line. If not, let A'B' meet AB in C'': then C'' may be written for C' in the hypothesis, whence AC': C'B :: AC'': C''B, and either C' and C'' coincide, or they are conjugate points in AB. In the last alternative C'' is internal or external, according as C' is external or internal: but A', B', C' have by hypothesis, an odd number of externals, therefore A', B', C'' (in a straight line) have an even number or none, which is absurd.

(8.) If O be any point, and OA, OB, OC, cut BC, CA, AB in A', B', C', then an *odd* number of sides is cut *internally*, (AC': C'B)(BA': A'C)(CB': B'A) = 1:1. This compound, by (7.), is (AOC: COB)(BOA: AOC)(COB: BOA), or 1:1. Treat the converse as before.

(9.) Let A'B'C' be a triangle having its vertices on the sides BC, CA, AB of the triangle ABC, and let A'B', B'C', C'A' cut AB, BC, CA, in C'', B'', A''; so that BC is cut in A' and A'', &c. Then the division is internal and external in *all*, or in *none*. Let A'B'C' be called an *inscript* of ABC, and ABC a *descript* of A'B'C'.

* Even in drawing without ruler and compasses, this method will be found a useful assistant to the eye.

(10.) The three sides, AB, BC, CA, are anharmonically* divided in the same manner, giving $(AC'BC'') = (BA'CA'') = (CB'AB'')$. The transversals passing through A' show that $CA' : A'B$ is both $(CB'' : B'A)(AC' : C'B)$ and $(CB' : B'A)(AC'' : C''B)$. A very easy form of the proof is this: each of the ratios $(AC'BC'')$ &c. is nothing but the compound $(AC' : C'B)(BA' : A'C)(CB' : B'A)$. Converses may be made.

(11.) If two lines meeting in Z , be cut by three lines meeting in O , in A, B, C, A', B', C' , then they have similar anharmonic divisions, and $(ZABC) = (ZA'B'C')$. Draw $AB', A'B$, &c. and there are six cases of inscription; $A'BO$ and $AB'O$ are inscripts of ZCC' ; $B'CO$ and $BC'O$ of ZAA' ; $C'AO$ and $AC'O$ of ZBB' : from which the theorem follows instantly, and, of course, its extension to any pencil of four lines and any pair of transversals. The particular case, where the anharmonic ratios become those of equality, must of course be noted. Also, that if two similar anharmonic systems have one point in common, the lines joining the points of the three corresponding couples meet in one point. (12.) Notice that $A''B''C''$ is an inscript both of ABC and $A'B'C'$; and call it the *common inscript*; or, when $A''B''C''$ are in one straight line, an *evanescent inscript*. (13.) When the inscript is *harmonic*, that is when the sides of ABC are harmonically divided by those of $A'B'C'$, then AA', BB', CC' meet in one point: and the converse. (14.) When the inscript is harmonic, the common inscript is evanescent; and the converse.

(15.) The complete quadrilateral is a triangle and a transversal, in four different ways. If, as before, ABC be the triangle, and $A'B'C'$ the transversal, then A, A' are opposite points, as are B, B' , and C, C' . The *triad* of quadrilaterals is $ABA'B', BCB'C', CAC'A'$. Draw the diagonals AA', BB', CC' , and let AA', BB' meet in C'' ; BB', CC' , in A'' ; CC', AA' , in B'' . In the figure are seven lines, and $ABA'B'$ and its attendants are *complete*. But there are six other systems of quadrilaterals, each of which is incomplete, wanting one diagonal, and dispensing with one of the seven lines. Thus $AA'BB'$ has the diagonals $AB, A'B'$, wants CC'' , and has nothing to do with CC' . Throw out the line CC' , replace

* I take this theorem to be new, which I should not have done from its being new to me when I found it: but on communicating it to Mr. T. S. Davies, whose research in this subject is far above mine, I found that it was equally new to him. But I shall not definitively call it new till I hear what M. Chasles says on the subject. I may add, that the substitution of (10.) for the celebrated proposition of Pappus in (11.) is the joint work of Mr. Davies and myself. On my communicating (10.), he returned me a demonstration of it by means of (11.), from which I saw that (11.) was really a case of (10.)

it by CC'' , and $AA'BB'$ is complete, while $ABA'B'$, which was complete, takes its place among the incomplete ones. As follows :

AA'BB' has diagonals AB, A'B' wants CC'' and dispenses with CC' .					
BB'CC'	...	BC, B'C'	...	AA''	...
CC'AA'	...	CA, C'A'	...	BB''	...
AA'B'B	...	AB', A'B	...	C'C''	...
BB'C'C	...	BC', B'C	...	A'A''	...
CC'A'A	...	CA', C'A	...	B'B''	...
					AA'
					BB'

(16.) That each diagonal is harmonically divided by the other two is well-known : but this is only a very small part of the following. Take any two points and their opposites, say A, C, A', C'. Any triangle made from *three* of these, say ACC', has BB'B'' for an harmonic inscript, as proved by the lines which meet in the *fourth* point, A'. This gives twelve cases of harmonic inscription. (17.) Let A''B''C'' be called the *diagonal triangle*: it has four harmonic inscripts. Of the remaining six letters take any three which are in the same straight line, say A'BC. The other three AB'C', show an harmonic inscript of the diagonal triangle, A'BC, first named, being the evanescent common inscript.

(18.) Hence it follows that A''A, B''B, C''C, &c. meet in one point. Let them meet in 0; A''A, B''B, C''C', in 1; A''A', B''B, C''C', in 2; A''A', B''B', C''C in 3. There are then six new lines 01, 12, 23, 30, 02, 31. If C''A'' be taken as a seventh line, then 0123 is complete; if B''C'', then 1203; if A''B'', then 2013.

The above will be sufficient for my purpose, and will show, I think, the value of the *anharmonic ratio*. It will be observed that I adhere closely to the language of geometry, and do not admit that of algebra: but I go further, and, while thinking of the subject, do not admit the *notions* of arithmetic. From various writers I gather that they think, in compounding the ratio, say of AB to CD and PQ to RS, there is no choice except either to compare the rectangles under AB and PQ and under CD and RS, or else the products $AB \times PQ$ and $CD \times RS$, the linear symbols being interpreted numerically: so that the composition of three linear ratios in plane geometry is resisted as involving the use of solids. But if any one will accustom himself to fix his mind upon *alteration in a ratio* as an operation which can be conceived independently of number, and executed independently of rectangles, he will find that the propositions of the geometry of transversals, &c. have a vitality which the algebraic forms cannot give them. The following process would not only help the confirmed algebraist to receive

the notion for which I contend, but would be a useful exercise in drawing. Form a triangle ABC, and a transversal A'B'C', draw any line K, and make an attempt by unassisted estimation to alter K into L in the ratio of AC' : C'B : and then cover K over with a bit of paper. By estimation again, draw M so that L : M as BA' : A'C, and N so that M : N as CB' : B'A. Then N and K should be equal. In all this there is neither occasion to think of a solid, nor of an area, nor of a numerical product.

XXIV. *On the Inorganic Constituents of Organic Bodies.* By H. ROSE, *Professor of Chemistry in the University of Berlin* *.

[Continued from p. 24.]

[The following are the Appendices referred to in the preceding portion of Prof. Rose's paper, at p. 4 *et seq.* of the present volume. They have been slightly, but not materially, abridged, and principally only in regard to the details of such analytical methods of examination as have yielded unfavourable results in the hands of the author, or such facts as have been described in the preceding part.]

APPENDIX I. and II.

Examination of the Inorganic Constituents of Peas and Pea-straw. By M. Weber.

300 grms. of peas and 100 grms. of pea-straw were used. After carbonization the mass was treated according to the method previously described.

The aqueous extract, when evaporated to dryness, furnished in the case of the peas, 1·140 grm.; in the case of the pea-straw, 1·417 grm.

These residues gave the following results per cent.—

	Peas.	Pea-straw.
Chloride of potassium	47·54	7·14
Chloride of sodium	8·16	6·65
Potash	30·26	57·10
Lime	0·70
Phosphoric acid	4·47	...
Sulphuric acid	0·79	2·12
Carbonic acid	8·33	23·12
Silica	1·48
	<hr/> 99·55	<hr/> 98·31

These constituents, when calculated as salts, yield the following composition:—

* From Poggendorff's *Annalen*, lxxvi. p. 338.

	Peas.		Pea-straw.
K Cl . . .	47.54	K Cl . . .	7.14
Na Cl . . .	8.16	Na Cl . . .	6.65
3KO + P ₂ O ₅	13.32	KO, SO ₃ . .	2.46
KO, SO ₃ . .	1.72	KO, CO ₂ . .	72.62
KO, CO ₂ . .	26.16	KO, SiO ₃ . .	4.49
KO, HO . .	3.13	KO, HO . .	3.88
	<hr/> 100.03	CaO, SO ₃ . .	1.69
			<hr/> 98.93

Thus the chlorides exist in the aqueous extract of the carbonized peas in far larger quantity than in that of the pea-straw. Phosphoric acid exists only in the peas; it is entirely absent from the aqueous extract of the carbonized pea-straw; whilst the latter contains a much larger quantity of carbonate of potash than the carbonized peas, so that the evaporated aqueous extract of the carbonized pea-straw would have exactly the same composition as several of the commercial kinds of potash.

The aqueous extract of both the carbonized peas and the pea-straw contained some, although not a large quantity, of free potash, formed by the above-mentioned action of the carbon upon carbonate of potash. As we have already stated, the conversion of the potash into the carbonate by the transmission of carbonic acid through the solution was omitted in all the analyses. Hence it is calculated as potash in the evaporated aqueous extract.

Muriatic extract.—The carbonized peas after exhaustion with water did not evolve carbonic acid when treated with muriatic acid; the carbonized straw, however, effervesced strongly, and hence contained a large quantity of earthy carbonates. As the amount of carbonic acid could not be determined directly, the lime and magnesia not precipitated from the muriatic solution by ammonia in the form of earthy phosphates are calculated as carbonates.

The composition of the constituents in the two extracts was as follows:—

	Peas.	Pea-straw.
Potash	54.63	
Soda	7.56	
Carbonate of lime	60.19
Carbonate of magnesia	5.26
Lime	8.22	6.92
Magnesia	6.52	5.69
Peroxide of iron . . .	1.33	1.13
Phosphoric acid . . .	20.79	18.29
Silica	0.95	2.52
	<hr/> 100.00	<hr/> 100.00

The large amount of potash in the acid extract of the carbonized peas is very remarkable. The quantity of phosphoric acid present is about sufficient to form with the lime and magnesia 2CaO , P_2O_5 , and 2MgO , P_2O_5 ; the calculated quantity of phosphoric acid would then amount to 21.92 per cent. Were it even admitted that the alkalis had formed with the two earthy phosphates, compounds of *c*-phosphoric acid insoluble in water, the quantity of alkali would be far too great for this purpose; for the potash alone requires more phosphoric acid than was found to form 3KO , P_2O_5 .

The amount of phosphoric acid found in the acid extract of the carbonized pea-straw is exactly sufficient to form *b*-phosphates with the lime and magnesia; the calculated percentage of phosphoric acid would then amount to 18.53. But both the salts undoubtedly exist in the carbonized residue in the form of *c*-phosphates, and the amount of the carbonates of lime and magnesia must be less by so much as is requisite to form *c*-phosphoric acid from the *b*-phosphates.

Remaining carbonaceous mass.—The following is the composition per cent. of the ash obtained by incinerating the exhausted carbonaceous mass of the peas and pea-straw.

	Peas.	Pea-straw.
Potash	24.14	
Lime	5.25	1.87
Magnesia	11.22	14.66
Peroxide of iron . .	0.85	7.73
Phosphoric acid . .	58.03	20.80
Silica	0.51	54.94
	<hr/> 100.00	<hr/> 100.00

Considering the large quantity of phosphoric acid in the ash of the peas, we must admit that one portion of the bases is combined with *a*-phosphoric acid, and another with *b*-phosphoric acid. If we calculate the lime and magnesia, as also the small quantity of peroxide of iron as *b*-phosphates (which certainly ought not to be done in the case of the peroxide of iron), 26.08 per cent. of phosphoric acid are required. If we assume that the remaining 31.95 per cent. of phosphoric acid are combined with the potash to form KO , P_2O_5 , we require 21.09 per cent. of potash. The quantity found is 3.05 per cent. more than this.

For the purpose of showing the important difference which occurs when the incineration of the exhausted carbonized mass is effected by the aid of a solution of platinum, or by my former method, by combustion in oxygen, the result of the examination of the ash of this substance may be brought for-

ward here, as obtained by the latter defective method. In this case it amounted to 1·424 grm. from 300 grms. of peas, whilst by the other method 2·726 grms. were obtained.

It consisted per cent. of—

Potash	17·83
Lime	8·77
Magnesia	20·85
Peroxide of iron . . .	2·24
Phosphoric acid . . .	46·98
Silica	1·89
	<hr/>
	98·56

The amount of the potash and phosphoric acid is diminished by the volatilization of the phosphate of potash; whilst that of the lime, the magnesia, the peroxide of iron, and the silica have been increased.

The proportion of the phosphoric acid to the bases in the ash of the pea-straw is the same as that in the *b*-phosphates. Yet only lime and magnesia could have been combined with the *b*-phosphoric acid, since the quantity of this acid found is only sufficient for these bases. If the two earths were considered as *b*-phosphates, 21·60 per cent. phosphoric acid would be required. The considerable quantity of peroxide of iron must not be considered as existing in combination with phosphoric acid in the ash.

The following are the inorganic constituents contained in the muriatic extract and the incinerated carbonized residue of peas; they have been added together, because they are sometimes considered as constituting the portions of the ash insoluble in water.

Potash	32·66
Soda	2·11
Lime	6·08
Magnesia	9·91
Peroxide of iron . . .	0·98
Phosphoric acid . . .	47·62
Silica	0·64
	<hr/>
	100·00

We thus see the large amount of alkali contained in those constituents which are insoluble in water, and the presence of which has previously been almost always overlooked.

The inorganic constituents of the muriatic extract and incinerated carbonized mass of pea-straw were—

Carbonate of lime . . .	54.29
Carbonate of magnesia . .	4.77
Lime	6.42
Magnesia	6.58
Peroxide of iron . . .	1.78
Phosphoric acid . . .	18.52
Silica	7.64
	<hr/>
	100.00

The following is the result of the analysis of the inorganic constituents of peas:—

In the aqueous extract of the carbonized peas	23.61 per cent.
In the muriatic extract	21.48 ...
In the ash of the carbonized mass . . .	55.36 ...
	<hr/>
	100.45

The total amount of inorganic constituents of the peas was 1.64 per cent.

Hence the constituents obtained in the three operations were—

		Oxygen.	
Chloride of potassium . . .	11.02		
Chloride of sodium . . .	1.89		
Potash	32.15	5.44	} 10.33
Soda	1.62	0.41	
Lime	4.67	1.31	
Magnesia	7.62	2.94	
Peroxide of iron . . .	0.76	0.23	
Phosphoric acid . . .	37.67	21.10	} 22.84
Sulphuric acid . . .	0.18	0.10	
Carbonic acid . . .	1.94	1.39	
Silica	0.48	0.25	
	<hr/>		
	100.00		

The pea-straw yielded—

Extracted by water from the carbonized mass	27.00 per cent.
Extracted by muriatic acid	65.87 ...
Ash of the residuary carbonized mass . . .	7.13 ...
	<hr/>
	100.00

The total amount of inorganic constituents of the pea-straw amounted to 5.25 per cent.

The constituents obtained in the three divisions of the analysis yielded—

		Oxygen.	
Chloride of potassium	1·96		
Chloride of sodium	1·83		
Potash	15·68	2·65	} 13·18
Lime	27·14	7·62	
Magnesia	6·50	2·51	
Peroxide of iron	1·30	0·40	
Phosphoric acid	13·52	7·57	} 26·37
Sulphuric acid	0·57	0·34	
Carbonic acid	25·52	18·46	
Silica	5·98		
	<hr/>		
	100·00		

The proportion of the oxygen of the bases to that of the acids in both the peas and the pea-straw is therefore as 1 : 2. The oxygen of the silica in the pea-straw is not added to that of the other acids, because it is not combined with bases in the straw.

The manner in which the inorganic constituents of organic substances are usually determined, consists in the direct incineration of the organic substance, and in arranging the constituents found in the analysis, by uniting the strongest bases with the strongest acids. The carbonic acid in many cases was not determined directly; and the portion of the bases remaining, after calculating the salts formed by them with the acids found, was assumed as carbonates. The above results show to what very erroneous ideas this method of arrangement may lead. On calculating according to the above principle the salts from the numbers quoted last, we should obtain totally different arrangements, or at least totally different per-centage results from those obtained by the separate exhaustions of the carbonized substance. Thus all the alkali, for instance, in the ash of the peas remaining after calculating the amounts of chloride and alkaline sulphate, would be regarded as alkaline phosphate. The phosphoric acid then remaining would be combined with other bases, and what remained of the latter would be regarded as combined with carbonic acid. But we have now seen that part of the potash exists in the aqueous solution of the carbonized mass in the state of carbonate, and that in the teleoxidic portion of the carbonized mass the earths extracted by the muriatic acid could only have been combined with phosphoric acid, because no carbonic acid is evolved when an acid is added.

This principle is seen to be still more erroneous in the determination of the inorganic constituents of the pea-straw. No phosphoric acid exists in the aqueous extract of the car-

bonized mass, but almost all the potash is combined with carbonic acid, whilst the earths in the teleoxidic portion of the carbonized mass are mostly combined with carbonic acid, and partly with phosphoric acid.

The fact that organic substances, the ash of which consists principally of earths, may be very easily incinerated, whilst those which contain a large amount of alkalies are very difficultly so, forms an important objection to the complete incineration of the organic substance for the determination of the amount of ash contained in it. For by the prolonged heat required in the latter case, the greater portion, and frequently the whole of the alkaline chlorides, especially the chloride of potassium, is volatilized; or by the action of *b*-phosphates with the aid of water, or by the hydrogen evolved and the oxygen of the air, muriatic acid is liberated, and they are thus converted into *c*-phosphates. Carbonic acid is driven off in the same manner. For this reason, in the incineration of those organic substances which abound in alkalies, Wackenroder has proposed to mix them with a weighed quantity of an earth, to prevent their fusion and to promote the ready combustion of the carbon.

APPENDIX III. AND IV.

Examination of the Inorganic Constituents of Rape-seed and Rape-straw. By M. Weber.

Aqueous extract.—The following are the per-centage results:—

	Rape-seed.	Straw.
Chloride of potassium	1·39	3·82
Chloride of sodium	9·71
Potash	67·88	58·91
Lime	0·32
Phosphoric acid	12·84	
Sulphuric acid	2·08	1·86
Carbonic acid	11·11	25·32
Silica	1·21	1·47
	96·51	100·41

On calculating the salts from the constituents obtained, a very large amount of free potash remains in excess, which existed in the evaporated residue as hydrate of potash. The apparently considerable loss arises from the water of the hydrate of potash not having been given among the constituents. Moreover, on account of the large amount of fatty oil in the seeds, a very large quantity of olefiant gas was evolved during

the carbonization, and this caused the conversion of so large an amount of carbonate of potash into potash.

The above constituents, when calculated as salts, yield the following composition :—

	Rape-seed.
Chloride of potassium	1·32
Tribasic phosphate of potash	38·19
Sulphate of potash	4·51
Carbonate of potash	34·89
Tribasic silicate of potash	4·86
Hydrate of potash	15·10
	<hr/> 98·87

On calculating the hydrate of potash as the carbonate, the composition of the aqueous extract would be—

Chloride of potassium	1·35
Potash	66·28
Phosphoric acid	12·54
Sulphuric acid	2·03
Carbonic acid	16·61
Silica	1·19
	<hr/> 100·00

or

Chloride of potassium	1·35
Tribasic phosphate of potash	37·29
Sulphate of potash	4·41
Carbonate of potash	52·20
Tribasic silicate of potash	4·75
	<hr/> 100·00

The aqueous extract of the carbonized straw did not contain any free potash. The constituents of the extract, when calculated as salts, give the following composition :—

Chloride of potassium	3·82
Chloride of sodium	9·71
Sulphate of potash	3·06
Carbonate of potash	79·53
Tribasic silicate of potash	4·46
Sulphate of lime	0·77
	<hr/> 101·35

Muriatic extract.—The carbonized seeds, after exhaustion with water, did not effervesce on the addition of muriatic acid; the straw, however, did so copiously.

The composition in 100 parts was as follows:—

	Seed.	Straw.
Potash	30·45	
Soda	4·48	
Carbonate of lime	63·38
Carbonate of magnesia	20·46
Lime	8·06	4·45
Magnesia	14·34	0·46
Peroxide of iron	1·36	2·13
Phosphoric acid	40·63	5·93
Sulphuric acid	0·32	
Silica	0·36	3·19
	100·00	100·00

The composition of the phosphates precipitated by ammonia from the muriatic solution was as follows, after having been heated to redness:— $8\text{CaO} + \text{P}_2\text{O}_5 + 2\text{MgO} + \text{P}_2\text{O}_5 + 2\text{Fe}_2\text{O}_3 + 3\text{P}_2\text{O}_5$. The calculated quantity of phosphoric acid in this precipitate amounts to 34·24 per cent.; analysis yielded 34·47 per cent. Thus there is an excess of 6·16 per cent. phosphoric acid.

The phosphates precipitated by ammonia from the acid extract of the straw consisted, after having been heated to redness, of 2CaO , $\text{P}_2\text{O}_5 + 2\text{MgO}$, $\text{P}_2\text{O}_5 + \text{Fe}_2\text{O}_3$.

The remaining carbonaceous mass.—The ash it contained consisted of—

	Seed.	Straw.
Potash	21·50	
Soda	0·29	
Lime	15·19	27·53
Magnesia	14·08	13·51
Peroxide of iron	0·46	4·20
Phosphoric acid	45·79	9·29
Sulphuric acid	1·87	
Silica	0·82	45·47
	100·00	100·00

The composition of the phosphates precipitated by ammonia from the acid solution of the ash of the seeds was 2CaO , $\text{P}_2\text{O}_5 + 2\text{MgO}$, $\text{P}_2\text{O}_5 + \text{Fe}_2\text{O}_3$, $3\text{P}_2\text{O}_5$. The phosphoric acid required by calculation is 44·17 per cent.; that found by experiment amounted to 44·10 per cent. Hence there is an excess of 1·69 per cent. phosphoric acid, which existed in the liquid filtered from the earthy phosphates. Ammonia however produced only a very slight precipitate in the acid solution of the ash of the carbonized straw; the fluid filtered from this still contained lime and magnesia.

The following results were obtained in the three divisions of the examination of the inorganic constituents of the rape-seed and rape-straw:—

	Seed.	Straw.
Extracted by water from the carbonized mass	9.50 p. c.	39.59 p. c.
Extracted by muriatic acid	35.60 p. c.	45.91 p. c.
In the ash of the residue	54.90 p. c.	14.50 p. c.
	<hr/> 100.00 p. c.	<hr/> 100.00 p. c.

The inorganic constituents of the rape-seed amounted to 2.48 per cent., those of the straw to 3.93 per cent.

Hence the following are the whole of the inorganic constituents of the rape-seed:—

		Oxygen.	
Chloride of potassium	0.13		12.86
Potash	28.94	4.90	
Soda	1.75	0.44	
Lime	11.19	2.33	
Magnesia	12.84	4.97	
Peroxide of iron	0.74	0.22	25.15
Phosphoric acid	40.79	22.85	
Sulphuric acid	1.34	0.80	
Carbonic acid	1.58	1.14	
Silica	0.70	0.36	
	<hr/> 100.00		

The amount of oxygen in the acids is twice as great as that of the bases in the rape-seed as in peas.

The rape-straw yielded the following inorganic constituents:—

		Oxygen.	
Chloride of potassium	1.48		13.29
Chloride of sodium	3.79		
Potash	23.02	3.90	
Lime	22.47	6.31	
Magnesia	6.72	2.60	
Peroxide of iron	1.57	0.48	27.10
Phosphoric acid	4.07	2.28	
Sulphuric acid	0.76	0.45	
Carbonic acid	27.51	19.90	
Silica	8.61	4.47	
	<hr/> 100.00		

Two analyses were also made of rape according to the old plan, and were found to yield the same errors as those which have been previously described.

On comparing the constituents of the ash of peas with those of rape-seed, we find that the aqueous extract of the carbonized peas differs essentially from that of the carbonized rape in the large amount of alkaline chlorides which it contains, and which are present in very small quantity in the rape, whilst the latter contains more phosphate of potash than the former. The muriatic extract of the carbonized rape also contains much more phosphoric acid than that of the peas, whilst the anoxidic portion of the carbonized mass of the peas yields more phosphoric acid than that of the rape. The inorganic constituents of the pea- and the rape-straw are however very similar to each other.

APPENDIX V.

On the Amount of Silica contained in some Plants.

By M. Struve.

As is well known, the stems of the *Equisetaceæ* when incinerated leave a residue consisting of almost pure silica, which retains the form of the original stem. It fuses before the blowpipe on charcoal. If this residue be exhausted, first with water and then with muriatic acid, which dissolve very small quantities of alkaline salts, among which we find phosphoric acid, together with sulphate and phosphate of lime, it no longer fuses before the blowpipe. The ash of the following species of *Equisetum*, after treatment with water and acids, yielded the following composition:—

	Silica.	Alumina.	Lime.	Impure manganoso- manganic oxide.
<i>Equisetum hiemale</i>	97·52	1·700	0·69	
<i>Equisetum limosum</i>	94·85	0·990	1·57	1·69
<i>Equisetum arvense</i>	95·43	2·556	1·64	

The epidermis of the stolones of *Calamus Rhodan*, the Spanish cane, which may be easily separated by repeatedly bending them, consists almost entirely of silica, whilst the woody substance itself contains mere traces of it. Muriatic acid does not remove either alumina or other bases, excepting a very small quantity of lime. This silica does not fuse before the blowpipe. After treatment with muriatic acid, it had the following composition:—

Silica	. . .	99·20
Lime	. . .	0·45
		<hr/> 99·65

Spongia lacustris, from the neighbourhood of Berlin, also

leaves an ash having the form of the plant, which, as in the preceding instances, is not destroyed after exhaustion with water and muriatic acid, which removes some lime. Its composition was then as follows:—

Silica . . .	94·66
Alumina . .	1·77
Lime . . .	2·99
	<hr/>
	99·42

It is very probable that the substance stated to be alumina in these analyses is really phosphate of lime or magnesia. Alumina has not hitherto been detected with certainty in the ash of plants.

APPENDIX VI. and VII.

Examination of the Ash of Wheat and Wheat-straw. By M. Weber.

The quantity of substance used amounted to 300 grms. in the case of the grain, and 100 grms. in that of the straw.

Aqueous extract.—During evaporation it deposited copious films of silica. That of the grain left a residue weighing 1·413 grm., that of the straw 1·216 grm.; it had the following composition:—

	Wheat-grain.	Straw.
Chloride of potassium	48·09
Chloride of sodium . .	27·05	2·84
Potash	33·64	2·17
Soda	6·37	
Sulphuric acid	2·34
Phosphoric acid . . .	31·72	
Silica	44·58
	<hr/>	<hr/>
	98·78	99·92

The presence of so large a quantity of silica in the aqueous extract of the straw is very remarkable. A soluble compound of chloride of potassium and silica must be formed under certain circumstances. The films of silica deposited during evaporation, were not changed by digestion with muriatic acid; nor did they exhibit any regular structure under the microscope, but consisted of pure silica.

Potash and soda exist in the aqueous extract of the carbonized wheat in combination with phosphoric acid in the form of *b*-phosphates. The amount of phosphoric acid required by calculation is 32·72 per cent.

Muriatic extract.—The residue consisted of—

	Wheat.	Wheat-straw.
Potash	14.40	
Soda	1.66	
Lime	4.33	46.83
Magnesia	22.35	10.56
Peroxide of iron	1.72	2.96
Phosphoric acid	54.77	33.54
Silica	0.77	6.11
	<hr/> 100.00	<hr/> 100.00

In the muriatic extract of the wheat, all the bases combined with phosphoric acid are in the form of *b*-phosphates. The quantity of phosphoric acid corresponding to these salts amounts to 57.20 per cent. ; experiment gave only 54.77 per cent.

In the muriatic extract of the carbonized straw, the precipitate produced by ammonia consisted of $8\text{CaO} + 3\text{PO}_5 + 2\text{Fe}_2\text{O}_3 + 3\text{P}_2\text{O}_5$; the phosphoric acid thus required amounts to 34.60 per cent. ; analysis yielded 33.54 per cent. The liquid filtered from this precipitate still contained small quantities of the carbonates of lime and magnesia.

The exhausted carbonized mass.—The composition of the ash was as follows :—

	Wheat-grain.	Straw.
Potash	22.70	
Lime	7.30	1.97
Magnesia	9.86	0.66
Peroxide of iron	1.76	1.13
Phosphoric acid	54.05	1.54
Silica	4.33	94.70
	<hr/> 100.00	<hr/> 100.00

On calculating the bases in the ash of the residuary carbonaceous mass as *b*-phosphates, there is an excess of phosphoric acid, part of which formed *a*-phosphates. The bases would require 44.19 per cent. phosphoric acid to form *b*-phosphates. The principal constituent of the ash of the straw was silica. The muriatic solution of the ash yielded with ammonia a precipitate consisting of $8\text{CaO} + 3\text{P}_2\text{O}_5 + 2\text{MgO} + \text{P}_2\text{O}_5 + \text{Fe}_2\text{O}_3 + \text{P}_2\text{O}_5$. The liquid filtered from this precipitate still contained small quantities of lime and magnesia, probably more than would correspond to the solubility of the earthy phosphates in a solution of muriate of ammonia. The three divisions of the analysis of the grains and straw of wheat gave—

	Grain.	Straw.
Extracted by water	36·80	31·79
Extracted by muriatic acid	43·93	13·39
Ash of the remaining carbonaceous mass	19·27	55·82
	<hr/> 100·00	<hr/> 100·00

The inorganic constituents of the grain amount to 1·28 per cent.; those of the straw to 3·825 per cent.

Thus the composition of the entire ash of the grain would be—

		Oxygen.
Chloride of sodium	10·00	
Potash	23·18	3·83
Soda	3·09	0·76
Lime	3·33	0·83
Magnesia	11·75	3·54
Peroxide of iron	1·11	0·34
Phosphoric acid	46·36	25·97
Silica	1·18	
	<hr/> 100·00	

The oxygen contained in the bases to that in the phosphoric acid is nearly in the proportion of 2:5. It has already been stated that the phosphates obtained in the various parts of the analysis were pyrophosphates.

The composition of the entire ash of the straw is—

		Oxygen.
Chloride of potassium	15·13	
Chloride of sodium	0·89	
Potash	0·68	0·11
Lime	6·93	1·94
Magnesia	1·69	0·65
Peroxide of iron	0·99	0·30
Phosphoric acid	5·05	2·82
Sulphuric acid	0·74	0·44
Silica	67·90	
	<hr/> 100·00	

As we have already stated, wheat-straw belongs apparently to the *meroxidic* substances. But if we deduct the large amount of silica, which must be regarded as existing in a perfect state of oxidation in the straw, from the anoxidic substances, so very small a quantity of it remains, that the wheat-straw may be regarded as an almost *teleoxidic* substance.

APPENDIX VIII.

Analysis of the Ash of the Blood of the Ox. By M. Weber.

The entire blood was carbonized by the method stated.

Aqueous extract.—The blood requires to be washed for nearly fourteen days to obtain a proper extract for the first portion of the analysis.

The residue of the aqueous extract consisted of—

Chloride of sodium	59.31	} or {	Na Cl . . .	59.31
Soda	14.67		KO, SO ₃ . .	0.78
Potash	11.91		3KO, P ₂ O ₅ . .	1.58
Phosphoric acid . .	0.53		KO, CO ₂ . .	15.31
Sulphuric acid . . .	0.36		NaO, CO ₂ . .	19.67
Carbonic acid . . .	13.01		NaO, HO . . .	4.05
	<hr/> 99.79			<hr/> 100.70

The small quantity of free alkali which is produced by the action of the carbon upon the alkaline carbonate, and which is considered as hydrate of soda, might perhaps be regarded more correctly as alkaline carbonate.

Muriatic extract.—This consisted of—

Soda	41.39
Potash	12.60
Lime	6.95
Magnesia	4.10
Peroxide of iron . . .	21.60
Phosphoric acid . . .	13.36
	<hr/> 100.00

The amount of alkali present is very striking, and is much too large to allow of the supposition that the alkalies combined with phosphoric acid had existed in the form of double salts with the earthy phosphates. The quantity of phosphoric acid found is exactly sufficient to form with the lime $8\text{CaO} + 3\text{PO}_5$, and with the magnesia $2\text{MgO} + \text{PO}_5$. The quantity required by calculation is 13.62 per cent.; that found amounts to 13.36 per cent. Probably the alkalies existed as chlorides in the carbonized mass which had been exhausted with water, and had escaped its action. This is rendered more probable by the fact, that the sum of the constituents of the muriatic extract, including the alkalies, amounted to much less than the direct weight of the evaporated residue.

Residuary carbonaceous mass.—It consisted of—

Potash	7.94
Soda	47.22
Lime	4.09
Magnesia	1.46
Peroxide of iron	16.69
Phosphoric acid	18.37
Sulphuric acid	0.61
Silica	3.62
	<hr/>
	100.00

The amount of phosphoric acid is too small to form *c*-phosphates with the earths and alkalies, much less can the peroxide of iron be considered as combined with phosphoric acid.

The relative amounts per cent. obtained by the three operations are—

Extracted by water	60.90
Extracted by muriatic acid	6.04
Ash of the residuary carbonaceous mass	33.06
	<hr/>
	100.00

The whole analysis of the blood gave—

		Oxygen.	
Chloride of sodium	36.16		
Soda	27.08	6.92	
Potash	10.66	1.80	
Lime	1.77	0.49	} 10.58
Magnesia	0.73	0.28	
Peroxide of iron	6.84	1.09	
Phosphoric acid	7.21	4.03	
Sulphuric acid	0.42	0.25	} 10.62
Carbonic acid	7.94	5.73	
Silica	1.19	0.61	
	<hr/>		
	100.00		

Hence the statement formerly made, that the inorganic constituents of the blood agree with those of many seeds, is only partly correct. The inorganic constituents of the wheat, excluding a considerable amount of chloride of sodium, consist almost entirely of pyrophosphates. The inorganic constituents of peas and rape, which differ in regard to the amount of alkaline chlorides they contain, nevertheless agree generally in the amount of the oxygen of all the bases being about half as great as that of the acids; whilst in the inorganic constituents of the blood the bases are combined with much smaller quantities of acids, so that the oxygen of the

base is about equal to that of the acids. There is especially a much less quantity of phosphoric acid to a larger amount of peroxide of iron in the blood, than in the seeds of the *Leguminosæ* or the *Cerealia*. At all events, the blood is a mercuridic substance; and the teleoxidic portion of it is only apparently greater than the anoxidic, because the large quantity of alkaline chlorides in the former cannot be considered as forming part of the teleoxidic portion.

[To be continued.]

XXV. On the Theory of the Tides.

By the Rev. BRICE BRONWIN*.

THE true principles of fluid motion were not known when Bernouilli and Euler produced their Theories of the Tides; and though Laplace in his Theory set out with the proper equations, he did not succeed in integrating them. After a very elaborate discussion of the subject, and arguing in a retardation, he concluded by merely making the height of the tide proportional to the disturbing forces of the sun and moon. There are two grand defects in all these theories. There is no retardation of the water resulting from the mathematical theory itself. If we set aside the contrivances to account for, or argue in a retardation, they give high water immediately under the luminary that raises it. And also they in reality make the direct action of the sun and moon to produce the whole of the effect; whereas it is admitted that in narrow seas at least their direct action produces no sensible effect, which indeed is evident from very obvious considerations.

It is upon the horizontal displacements of the water that the height of the tide and the retardation chiefly depend. But these have been neglected; in fact all the difficulty lies here. Laplace did not succeed in integrating his equations. The thing wanted is, to make δp , the variation of the pressure, a complete variation. Until this be done, we cannot expect to possess a theory which shall harmonize with the phænomena. What, therefore, I propose in this and one or two more papers is, not to give a complete theory of the tides, the phænomena of which are in a great measure out of my way, but to attempt to make δp a complete variation.

Laplace's theory is contained in the first and fourth books of the *Mécanique Céleste*, from which I shall take the neces-

* Communicated by the Author.

sary equations, retaining his symbols, which I suppose it will not be necessary to explain.

From book 4, chap. 1. sect. 4, we have

$$\begin{aligned}\alpha V = & \frac{L}{4r^3} \left(\sin^2 v - \frac{1}{2} \cos^2 v \right) (1 + 3 \cos 2\theta) \\ & + \frac{3L}{r^3} \sin \theta \cos \theta \sin v \cos v \cos (nt + \varpi - \psi) \\ & + \frac{3L}{4r^3} \sin^2 \theta \cos^2 v \cos 2(nt + \varpi - \psi). \quad . \quad . \quad . \quad (1.)\end{aligned}$$

This is the part of αV which depends on the action of the sun or moon. That which depends on the disturbing force of the water, arising from the deviation of its form from that of the equilibrium state, is insensible in small seas. And in these the deviation of the attraction of the land from the regular law of gravity will mostly be as great, or even greater, than that of the water; and its tendency will be in a considerable measure to annihilate the effects of the latter. We shall therefore neglect this force.

From book 1. chap. 8. sect. 36, we have, making the density unity and restoring p , which the author had made nothing,

$$\begin{aligned}& r^2 \delta \theta \left(\frac{d^2 u}{dt^2} - 2n \sin \theta \cos \theta \frac{dv}{dt} \right) \\ & + r^2 \delta \varpi \left(\sin^2 \theta \frac{d^2 v}{dt^2} + 2n \sin \theta \cos \theta \frac{du}{dt} \right) = -g \delta y - \delta p + \delta V.\end{aligned}$$

In this formula Laplace has very properly neglected the vertical displacement (s), which is quite insensible in comparison of the horizontal displacements (u) and (v). He has also left out the terms multiplied by δr , which quantity ought to be considered as of the same order with (s). If in this we put $r=1$, and make

$$\begin{aligned}\delta \omega = & \delta \theta \left(\frac{d^2 u}{dt^2} - 2n \sin \theta \cos \theta \frac{dv}{dt} \right) + \delta \varpi \\ & \left(\sin^2 \theta \frac{d^2 v}{dt^2} + 2n \sin \theta \cos \theta \frac{du}{dt} \right), \quad . \quad . \quad . \quad (2.)\end{aligned}$$

the above becomes

$$g \delta y + \delta p = \delta V - \delta \omega.$$

This, integrated, gives $gy + p = c + V - \omega$, where (c) is an arbitrary constant. It may, however, contain equations of long periods in (t), but cannot contain the angle $nt + \varpi - \psi$. Now

if we make $p=0$, this will belong to the exterior surface, and we shall have

$$gy=c+V-\omega, \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

which gives (y), the height of the tide.

From the place last referred to we have also

$$\frac{d(r^2s)}{dr} + r^2 \left(\frac{du}{d\theta} + \frac{dv}{d\omega} + \frac{u \cos \theta}{\sin \theta} \right) = 0,$$

which is the equation of continuity. In this equation the term $\frac{d(r^2s)}{dr}$ ought to have been left out in consistency with what the author had done in the other formula. The quantities (u) and (v) are many hundred, or even some thousand times larger than (s); it cannot therefore be allowable to express this exceedingly small quantity in terms of them. Leaving it out, and differentiating relative to (t), there results

$$\sin \theta \left(\frac{d^2u}{d\theta dt} + \frac{d^2v}{d\omega dt} \right) + \cos \theta \frac{du}{dt} = 0. \quad . \quad . \quad . \quad (4.)$$

By means of several hypotheses, very wide of the actual case of nature, Laplace has contrived to integrate this equation, with all its terms, relative to (r), and to make the terms in the result all of the same order of magnitude by the introduction of the depth of the sea into the larger ones as a factor. But such a result as this cannot be admitted with any tolerable regard to accuracy.

The condition that $\delta\omega$ may be a complete variation is

$$\frac{d}{d\omega} \left(\frac{d^2u}{dt^2} - 2n \sin \theta \cos \theta \frac{dv}{dt} \right) = \frac{d}{d\theta} \left(\sin^2 \theta \frac{d^2v}{dt^2} + 2n \sin \theta \cos \theta \frac{du}{dt} \right),$$

or

$$\begin{aligned} \frac{d^3u}{d\omega dt^2} - 2n \sin \theta \cos \theta \frac{d^2v}{d\omega dt} &= \frac{d}{d\theta} \left(\sin^2 \theta \frac{d^2v}{dt^2} \right) + 2n \sin \theta \cos \theta \frac{d^2u}{d\theta dt} \\ &+ 2n(1 - 2 \sin^2 \theta) \frac{du}{dt}, \end{aligned}$$

which by (4.) may be reduced to

$$\frac{d^3u}{d\omega dt^2} = \frac{d}{d\theta} \left(\sin^2 \theta \frac{d^2v}{dt^2} \right) - 2n \sin^2 \theta \frac{du}{dt}. \quad . \quad . \quad . \quad (5.)$$

From (4.) and (5.) we must determine u and v . To abridge, make $\phi = nt + \omega - \psi$. We shall take account of terms depending on this angle only; these in their most general form may be represented by $A \sin i\phi + B \cos i\phi$. But A and B , being functions of θ and t , may be developed in series, the single

terms of which will be of the form $A'\alpha$ and $B'\epsilon$, A' and B' being functions of θ only, and α and ϵ functions of t only. Therefore the values of u and v will be sums of terms of the form

$$A'\alpha \sin i\phi + B'\epsilon \cos i\phi = C(\alpha \sin i\phi + \epsilon \cos i\phi) \\ + D(\alpha \sin i\phi - \epsilon \cos i\phi),$$

if

$$C = \frac{1}{2}(A' + B'), \quad D = \frac{1}{2}(A' - B').$$

But the two terms in the second number of this equation may be put under either of the forms $A\alpha \sin i(\phi - \epsilon)$, or $A\alpha \cos i(\phi - \epsilon)$, which are therefore the most general possible. We may therefore make u and v , and also $\frac{du}{dt}$, $\frac{dv}{dt}$, equal to the sum of terms of this form; for the first general assumption applies to the differential coefficients equally with the quantities themselves.

Let then

$$\frac{du}{dt} = \Sigma A\alpha \sin i(\phi - \epsilon), \quad \sin \theta \frac{dv}{dt} = \Sigma B\beta \cos i(\phi - \epsilon).$$

These values substituted in (4.) and (5.) will give results of the form

$$M \sin i(\phi - \epsilon) + N \cos i(\phi - \epsilon) = 0.$$

Whence $M=0$, $N=0$, will be equations for determining A and B .

From (4.) we get

$$\left(\sin \theta \frac{dA}{d\theta} + \cos \theta A \right) \alpha - iB\beta = 0.$$

As θ and t are independent of each other, this equation cannot subsist unless one of them divide out. We must therefore have

$$\beta = \alpha, \quad \sin \theta \frac{dA}{d\theta} + \cos \theta A = iB. \quad . \quad . \quad . \quad (a.)$$

And there is no ambiguity; for whichever of these equations we assume, it leads to the other.

If we had made $\beta = k\alpha$, we should have had kB everywhere in the place of B ; therefore making $kB = B'$, the final result would not be altered.

Making the same substitution in (5.) which has been made in (4.), we find, putting

$$n - \frac{d\psi}{dt} - \frac{d\epsilon}{dt} = pn,$$

$$\left. \begin{aligned} ip \frac{d}{d\theta} (\sin \theta B) - (i^2 p - 2 \sin^2 \theta) A &= 0 \\ \left\{ \frac{d}{d\theta} (\sin \theta B) - iA \right\} \frac{d\alpha}{dt} &= 0 \end{aligned} \right\} \dots (b.)$$

The first of these has been divided by α , which multiplied all the terms. We cannot make in the second

$$\frac{d}{d\theta} (\sin \theta B) - iA = 0;$$

for this would reduce the first to $2 \sin^2 \theta A = 0$, which cannot be. We must therefore have $\frac{d\alpha}{dt} = 0$, and

$$\alpha = a, \text{ a constant. } \dots (c.)$$

Then, since A and B are not functions of t , the first of (b.) cannot subsist unless p be constant. We must therefore make

$$\frac{d\epsilon}{dt} + \frac{d\psi}{dt} = c\nu, \dots (d.)$$

where c is a constant, and ν is the mean motion of the planet, the first member being of this order.

If now we divide the first of (b.) by $p = 1 - c \frac{\nu}{n}$, neglecting the powers of $\frac{\nu}{n}$ above the first, we have

$$i \frac{d}{d\theta} (\sin \theta B) - (i^2 - 2 \sin^2 \theta - 2c \frac{\nu}{n} \sin^2 \theta) A = 0.$$

And if in this we neglect the very small quantity $2c \frac{\nu}{n} \sin^2 \theta A$, we have

$$i \frac{d}{d\theta} (\sin \theta B) - (i^2 - 2 \sin^2 \theta) A = 0. \dots (e.)$$

From (a.), (c.) and (d.), we have

$$\beta = \alpha = a, \quad \frac{d\epsilon}{dt} = c\nu - \frac{d\psi}{dt}. \dots (6.)$$

Eliminating B from (e.) by the second of (a.), there results

$$\sin^2 \theta \frac{d^2 A}{d\theta^2} + 3 \sin \theta \cos \theta \frac{dA}{d\theta} + (1 - i^2) A = 0. \dots (7.)$$

Particular integrals of this are, when $i = 2$,

$$A_2 = a_2 \left(1 + 2 \cos^2 \frac{\theta}{2} \right) \frac{\sin \frac{\theta}{2}}{\cos^3 \frac{\theta}{2}};$$

and when $i=1$, $A_1=a_1$, where a_2 and a_1 are the values of a from the first of (6.).

By means of these we easily find the other particular integrals. Let them be $A_2=b_2f(\theta)$, $A_1=b_1f_1(\theta)$. We shall find that $f(\theta)$ and $f_1(\theta)$ are infinite at the pole, and therefore inadmissible. Consequently we must have $b_2=0$, $b_1=0$, and

$$\left. \begin{aligned} A_2 &= a_2 \left(1 + 2 \cos^2 \frac{\theta}{2} \right) \frac{\sin \frac{\theta}{2}}{\cos^3 \frac{\theta}{2}}, & A_1 &= a_1 \\ B_2 &= \frac{1}{2} \frac{d}{d\theta} (\sin \theta A_2), & B_1 &= \frac{d}{d\theta} (\sin \theta A_1) \end{aligned} \right\}, \quad (8.)$$

where the values of B_2 and B_1 are derived from the second of (a.), and the arbitraries a_2 and a_1 are functions of r . It is to be observed that in making $\delta\omega$ a complete variation relative to θ and ϖ only, and not to r , we shall lose no terms depending on r ; for the arbitrary of the integral, not containing ϖ , and therefore not φ , must be rejected. And it is to be further observed, that there are no more solutions than that obtained.

Gunthwaite Hall, near Barnsley, Yorkshire,
July 28, 1849.

[To be continued.]

XXVI. *On the Chemical Composition of the Fluid in the Ascidia of Nepenthes.* By Dr. A. VOELCKER of Frankfort*.

THE watery secretions of certain plants belonging to the genera *Nepenthes*, *Cephalotus*, and *Sarracenia*, have long attracted the attention of botanists; but whilst the secreting organs of these plants have been minutely described, the chemical nature of the fluid itself has been but very imperfectly examined. That these liquids have not met with the attention to which their importance entitles them, may be accounted for by the circumstance that few chemists have an opportunity of obtaining the unaltered fluids, and that even those who are fortunate enough to procure them, seldom can command a sufficient quantity to enable them to investigate their nature. With the exception of Dr. Turner's analysis of the fluid in the ascidia of *Nepenthes*, I know of no other analysis of this fluid or of similar secretions. The botanists who have given attention to the subject of the watery secretions of the leaves of plants have found these secretions to consist in most cases of nothing but pure water, and have only occasionally dis-

* From the Annals and Magazine of Natural History, 2d Series, vol. iv. p. 128.

covered in them some vegetable matter. Treviranus for instance observed a tasteless water in the corolla of *Maranta gibba*, which he however did not further examine; the same gentleman examined the watery secretion of *Amomum Zerumbet*, and caused Dr. Göppert to subject it to chemical analysis, from which it resulted that the fluid between the scales of the spikes consisted of almost pure water, containing a small quantity of vegetable fibre and mucus.

The most remarkable instance of a watery secretion from the leaves of plants is recorded in the 'Annals of Natural History' for 1848, in a paper by Mr. Williamson, who observed that the leaves of *Caladium destillatorium* had the peculiar power of exhaling watery fluid from a point near the apex on the upper side. Each full-grown healthy leaf, according to Mr. Williamson's observation, produced about half a pint of water during the night, which, on being analysed, was found to contain a very minute portion of vegetable matter.

It appeared to me highly improbable that these fluid secretions should consist of pure water with merely a trace of vegetable matter, and no inorganic substances whatsoever. If they are to be regarded as true secretions, we naturally should expect them to contain some of the salts which we find in all juices of plants. I was therefore anxious to examine this point, and I am glad that I have an opportunity of bringing the results of my analysis of the fluid in the ascidia of *Nepenthes* before the notice of the Botanical Society. It is through the kindness of Prof. Balfour, Mr. Evans of the Experimental Gardens, Messrs. Jas. Dickson and Sons, and Sir W. Hooker, that I have obtained the materials for the following analysis, and I consider it my duty to express here publicly my deep sense of gratitude for the kindness and liberality with which the above-named gentlemen have assisted me in carrying on this inquiry. I have also to express my obligations to Dr. George Wilson for kindly allowing me the use of his laboratory.

Linnaeus regarded the ascidia of *Nepenthes* as a natural reservoir for rain, and thought that the water found in them was introduced from without, and was not secreted by the plant itself. His opinion however has been contradicted already by many botanists, especially Treviranus, who observed that the water in the pitchers of *Nepenthes destillatoria* is always clear, and that there exists a distinct secreting apparatus. Treviranus says, in an article which appeared in the 'Edinb. New Philosoph. Journal' for Oct. 1832—April 1833:—"The parietes of the leaf of *Nepenthes destillatoria* are traversed by a multitude of proportionally large anastomosing veins, which contain many true spiral vessels. The upper half of its inner surface is covered with a blue rind, as parts

often are which require to be protected from the action of water ; the under half is, on the contrary, shining and full of gland-like eminences directed downwards, and having a hole almost visible to the naked eye, which is uncovered by the cuticle which the remainder possesses." The watery secretion reaches generally to the level of these glands in the middle of the ascidium, and he thinks that they are true secreting organs. This peculiar structure alone gives a strong reason for thinking that the water in the ascidia of *Nepenthes* is supplied by the plant itself, and the circumstance that water is found in pitchers which have never been opened is another argument against the supposition that it comes from without. The subjoined analysis of the fluid moreover leaves no doubt that it is a true secretion.

Before I enter into the particulars of my experiments I will mention that I could not detect any oxalic acid in the fluid of *Nepenthes*. It is stated in Lindley's 'Vegetable Kingdom' that Dr. Turner found this acid in combination with potash, and that he also detected a trace of organic matter, which caused the watery fluid when boiling to emit an odour of boiled apples. Though I have examined the water of many pitchers from four different localities, and paid particular attention to the detection of oxalic acid, I have failed in finding a trace of it, and I am therefore inclined to believe that Dr. Turner, on account of the minute quantity of solid matter which he must have got on evaporation of the water, was unable to subject the minute crystals which he took for superoxalate of potash to a further examination, which would have shown him that the crystals were not superoxalate of potash, but chloride of potassium. The proportion of chloride of potassium which I found in the fluid is considerable ; it is deposited from the liquid after evaporation in the form of minute but very regular cubes. The odour of boiled apples which Dr. Turner observed I found very distinct when the water was heated to the boiling-point. Besides chloride of potassium I found malic and a little citric acid, in combination usually with soda, lime and magnesia, and a small quantity of another organic matter which gave a yellow tint to the water during its evaporation. The quantity of the latter was too minute to enable me to ascertain its chemical nature.

I will now proceed to describe the experiments with the different fluids in the ascidia of *Nepenthes* :—

1. Fluid from an unopened pitcher-plant grown in the Botanical Garden, Edinburgh.

The water which I got on the 12th of June, 1849, was perfectly colourless and clear ; it had an agreeable, not very pronounced smell and a refreshing taste. Though its taste was not sour, litmus paper showed the presence of an acid or an acid salt

by the red colour it assumed when dipped in the water. When heated it remained clear, and only assumed a slightly yellow colour when the liquid became very concentrated. The residue which remained on evaporation was cream-coloured, very hygroscopic, and dissolved entirely in a small quantity of distilled water. Litmus paper plunged in this solution was turned red immediately; the acid which is present in the water therefore was not volatilized during the evaporation.

The quantity of the water from one pitcher amounted to

17.41 grains,

which gave on evaporation

0.16 of dry residue, dried at 212° F.

100 parts of the fluid consequently contained

0.92 per cent. of solid matter.

2. Water from unopened pitcher-plants grown in the Botanical Garden, Edinburgh, June 13th, 1849.

The physical characters were the same as those of the preceding liquid. Litmus paper likewise was turned red when dipped in the water.

The behaviour of the water towards chemical tests was as follows :—

Ammonia produced no change.

Carbonate of ammonia produced no change.

Lime-water produced no change.

Chloride of calcium and ammonia produced no change.

Nitrate of barytes produced no change.

Nitrate of silver gave a white voluminous precipitate, insoluble in nitric acid, but soluble in ammonia.

Acetate of lead produced a white precipitate soluble for the greater part in boiling water.

Basic acetate of lead gave a white voluminous precipitate in the clear liquid filtered from the precipitate which was caused by neutral acetate of lead.

Oxalate of ammonia produced a small white precipitate of oxalate of lime.

Phosphate of soda and ammonia, added to the concentrated liquid filtered from the oxalate of lime, gave a crystalline white precipitate of phosphate of magnesia and ammonia.

Chloride of platinum, added to the water after having been evaporated to a small bulk, produced a crystalline yellow precipitate.

The residue left on evaporation of the water coloured the alcohol flame yellow.

These reactions indicate the presence of chlorine, potash, soda, magnesia, lime and organic acids, and prove the absence of other

bases and of sulphuric acid, tartaric acid, racemic acid, oxalic and phosphoric acid.

3. Fluid from unopened pitcher-plants grown in the Experimental Gardens, Edinburgh, June 13th, 1849.

The water was perfectly clear and colourless, had an acid reaction on litmus paper, and exhibited the same physical and chemical characters as the fluid from the pitcher-plants of the Botanical Garden.

63·21 grains of water left on evaporation a residue which, dried at 212° F., amounted to

0·58 grain.

100 parts of the fluid therefore contained

0·91 per cent. of dry residue.

Exposed to a red heat the residue (0·58 gr.) turned black, and gave off pungent fumes, and left a white ash after all the charcoal was completely burnt away, the weight of which was 0·42 of a grain.

The loss by burning therefore was 25·86 per cent.

The residue left on evaporation of this fluid was slightly coloured, and gave an almost colourless solution with water. A portion of this solution was kept in a closed bottle. After the lapse of a fortnight the water in the bottle became turbid and deposited some light white flakes. The acid reaction, which was very distinct before, had now disappeared entirely.

4. Fluid from opened pitcher-plants grown in the Experimental Gardens, June 14th, 1849.

The fluid in the open pitchers was coloured yellow, but otherwise perfectly clear. The reactions with chemical tests were the same as the preceding.

97·74 grains of water left on evaporation 0·85 of a grain of dry residue.

100 parts therefore contained 0·87 per cent. of solid matter.

This residue was coloured yellow, but redissolved entirely in a little water.

5. Fluid from unopened pitcher-plants grown in Messrs. Dickson's nursery, June 17th, 1849.

Fluid perfectly clear and colourless, reactions the same as above.

319·48 grains left a residue which, dried at 212° F., was found to weigh 1·88 grain; or

100 parts of the liquid contained 0·58 per cent.

6. Liquid from unopened pitcher-plants grown in Messrs. Dickson's nursery, June 21st, 1849.

Physical and chemical characters of the liquid the same as above.

193·82 grains of water left on evaporation 1·22 grain of dry residue, or 0·62 per cent.

When burnt the 1·22 grain lost in weight 0·44 of a grain, or 100 parts of the residue lost 36·06 per cent.

The solid matter of this liquid was very hygroscopic, and coloured more yellow than that of the Botanical and Experimental Gardens. I found that the total weight of the solid matter in this fluid was not so large as in that of the Experimental Gardens, but that the proportion of organic matter in the residue was larger than that in the residue of the fluid procured from the Experimental Gardens.

7. Water from opened pitcher-plants grown in Messrs. Dickson's nursery, June 24th, 1849.

This fluid was yellow-coloured and not quite clear. Litmus paper was turned red when moistened with the water. The reactions were the same as above, with the exception that nitrate of barytes produced a slight turbidity, indicating the presence of sulphuric acid. As I found no sulphuric acid in the liquid from the unopened pitchers of the same plants, nor in any of the liquids I examined, I think the sulphuric acid which I found must have resulted from the water with which the plants had been watered which had found its way into the open pitchers*. In order to see if the liquid contained any volatile acid, I subjected about half an ounce of it to distillation. The distillation was continued till the residue in the glass retort was evaporated to dryness, and the generated steam carefully condensed in a glass receiver. The distilled portion was perfectly pure water, and experienced no change by any reagent.

It results from this experiment that the liquid in the *ascidia* of *Nepenthes* does not contain any volatile acids, such as acetic or formic acid.

8. Fluid from unopened pitcher-plants grown in the Royal Gardens, Kew.

Having been unable to detect any oxalic acid in the above-mentioned fluids, I was anxious to ascertain whether or not the fluid of plants grown in other localities contained oxalic acid. I therefore applied to Sir W. Hooker, who with great liberality directed some liquid of unopened pitcher-plants grown in the Kew Royal Botanical Gardens to be sent to me. The physical and chemical characters of this fluid were precisely the same as those of the previously examined liquids. The proportion of solid matter it held in solution however was much smaller.

299·87 grains of the liquid left on evaporation only

0·82 of a grain of dry residue.

100 parts of the liquid therefore contained

0·27 per cent. of solid matter.

* The water in this instance was procured chiefly from the Water of Leith.

On burning, the 0·82 of a grain lost 0·27 of a grain, or 100 parts lost 32·92 per cent.

All the liquids from the different localities above-mentioned which were left over I mixed together and evaporated the mixture to dryness. One-half of the dry residue I exposed to a red heat, and used the remaining white ash for the determination of the inorganic salts of which it was composed.

The other half I dissolved in water and precipitated with basic acetate of lead, in order to obtain the organic acids in combination with lead. This precipitate I collected on a filter and washed with cold distilled water. It was then removed from the filter and suspended in water, through which a current of sulphuretted hydrogen was passed. By this means I separated the lead as sulphuret, and obtained the organic acids free dissolved in water. This solution was colourless and very acid; evaporated to a small bulk in a water-bath it assumed a yellow colour, and dried at last to a yellow crystalline mass, which deliquesced in the air and dissolved readily in water and alcohol, leaving behind a trace of a brown organic matter.

Lime-water added in excess to a portion of the acid solution produced no precipitate in the cold, but on boiling a small white precipitate fell down which redissolved entirely in sal ammoniac.

Chloride of calcium and ammonium left the liquid unchanged in the cold, but on boiling a precipitate was formed which was soluble in sal ammoniac.

Acetate of lead gave a white precipitate insoluble in ammonia, soluble in acetic acid.

Basic acetate of lead added to the liquid filtered from the precipitate caused by neutral acetate of lead produced another abundant white precipitate. From these reactions it appears that the precipitate with lime-water was caused by citric acid and not by tartaric or racemic acid, the reactions of which acids are similar to those of citric acid, for tartrate of lime is not soluble in sal ammoniac, whilst tartrate of lead redissolves readily in ammonia. Tartaric acid moreover is sufficiently characterized by the sparing solubility of its acid potash salt, and as the acid liquid did not give rise to the formation of such a salt with potash, we have another indirect proof of the presence of citric acid. A little tartaric acid added to the liquid in which tartaric acid was sought in vain, after a few minutes produced the sparingly soluble potash salt.

Racemic acid is thrown down both by lime-water and by a solution of gypsum; the acid liquid of *Nepenthes* remained unchanged by either reagent, hence it cannot have contained any racemic acid.

The precipitate caused by chloride of calcium and ammonia

and boiling was filtered hot, and alcohol and ammonia added to the clear liquid. The addition of alcohol produced a voluminous white precipitate, a reaction which indicates the presence of malic acid. The quantity of this precipitate was much larger than that of the lime precipitate which citric acid gave. The formation of a precipitate, upon addition of alcohol to the liquid from which the first had been separated by filtration, is characteristic of the presence of malic acid, for no other lime-salts were present; for instance, no sulphate of lime was present which could have produced a precipitate. But I thought it nevertheless necessary to examine the precipitate caused by the addition of alcohol further. When burnt it turned black, gave off pungent vapours, and was converted into carbonate of lime. The solution of chloride of calcium and ammonia used for the experiment remained clear after the addition of alcohol; the acid liquid likewise remained clear when alcohol was added; both put together immediately produced a white voluminous precipitate.

Basic acetate of lead, as already mentioned, throws down from the solution a white precipitate. I could not observe that this precipitate melted below the boiling-point of water, as pure malate of lead does, but it must be remembered that this reaction is distinctly marked only when the malate of lead is pure; admixtures of other salts of lead prevent it altogether; and as I have shown the presence of citric acid and another organic substance which is thrown down by basic acetate of lead, there can be no doubt that this was the reason why the precipitate did not dissolve in boiling water.

Though I have not been able to obtain a sufficient quantity of the acids of *Nepenthes* for an elementary analysis, I think the above reactions prove the presence of malic and citric acid. Oxalic acid, which is readily detected, as the weakest solution of an oxalate is thrown down by lime-water, I failed to discover; on the contrary, I have shown that the water contained lime, which excludes the co-existence of oxalic acid in a clear liquid. I have found that the smallest quantity of oxalic acid immediately caused the water of *Nepenthes* to become turbid.

The second half of the residue left on evaporation of the mixed fluids I exposed to a red heat in a platinum capsule. It turned black, gave off pungent fumes, and left a white salt after all the charcoal was burnt off.

On analysis this residue was found to consist of

Chloride of potassium	76.31
Carbonate of soda	16.44
Lime	3.94
Magnesia	3.94

The unburnt residue left on evaporation of the fluid in the ascidia of *Nepenthes* therefore consists, if we take the average of the loss of the three determinations at 31·61 per cent. and reject the carbonic acid of the ash, of—

Organic matter, chiefly	
Malic acid and a little citric acid	38·61
Chloride of potassium	50·42
Soda	6·36
Lime	2·59
Magnesia	2·59
	<hr/>
	100·57

It is remarkable that none of the fluids which I examined contained any sulphuric acid, which acid has been found in all juices of plants, and which I do not doubt also exists in the sap of *Nepenthes*. An ash analysis of this interesting plant would show the proportion of sulphuric acid at once; and as we are not in possession of an analysis of the ash of *Nepenthes*, which in other respects might be of interest, I take the liberty of asking those gentlemen who are in the possession of *Nepenthes*' plants to preserve the clippings of branches, &c., which I shall be glad to receive as materials for an ash analysis.

XXVII. *On Quaternions; or on a New System of Imaginaries in Algebra.* By Sir WILLIAM ROWAN HAMILTON, LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.

[Continued from p. 137.]

86. **T**HE same sort of quaternion analysis, proceeding from the formulæ in art. 82*, and from others analogous to them, has conducted the author to many other geometrical theorems, respecting the inscription of gauche polygons in surfaces of the second degree. An outline of some of these was given to the Royal Irish Academy in June 1849; and some of them may be mentioned here. To avoid, at first, imaginary† deformations, in passing from an original sphere,

* In art. 84, last line of page 135, for a tangential vector, read an arbitrary tangential vector.

In art. 85, fifth line from foot of page 136, for inscribed polygon of $2m$ sides, read inscribed polygon of $2m+1$ sides.

† While acknowledging, as the author is bound to do, the great courtesy towards himself that has been shown by several recent and able writers, on subjects having some general connexion or resemblance with those on which

the surface in which the polygons are inscribed shall be supposed, for the present, to be an *ellipsoid*. Results of the same *general* character, but with *some* important modifications, (connected with the *ordinary* square root of negative unity,) hold good for the inscription of such polygons in *other* surfaces of the same order, as the writer may afterwards point out. He is aware, indeed, that the corresponding class of questions, respecting the inscription of *plane* polygons in *conics*, has attained sufficient celebrity; and feels that his own acquaintance with what has been already done in that department of geometrical science is inferior to the knowledge of its history possessed by several of his contemporaries, for instance, by Professor Davies. He knows also that some of the published methods for inscribing in a circle, or plane conic, a polygon whose sides shall pass through the same number of given points, can be adapted to the case of a polygon formed by *arcs* of great circles on the surface of a sphere, and inscribed in a *spherical* conic; and he has, by quaternions, been conducted to some such methods himself, for the solution of this latter problem. But he acknowledges that he shall feel some little surprise, though perhaps not entitled to do so, if it shall turn out that the results of which he proceeds to give an outline, respecting the *inscription of rectilinear but gauche polygons in an ellipsoid*, have been wholly (or even partially) anticipated. They have certainly been, in his own case, results of the application of the quaternion calculus: but whatever geometrical truth has been attained by any one *general mathematical method* (such as the Quaternions claim to be), may also be found, or at least *proved*, by any *other* method equally general. And those who shall take the pains of *proving* for themselves, by

he has been engaged, he hopes that he may be allowed to say,—yet rather as requesting a favour than as claiming a right,—that he will be happy if the inventor of the *Pluquaternions* shall consent to his adopting or rather retaining a *word*, namely “biquaternion,” which the Rev. Mr. Kirkman has indeed *employed*, with reference to the *octaves* of Mr. J. T. Graves and Mr. Cayley, but does not appear to *want*, for any of his *own* purposes: whereas Sir W. Rowan Hamilton has for years been accustomed to use this word BIQUATERNION,—though perhaps hitherto without printed publication,—and indeed could not, without sensible inconvenience, have dispensed with it, to denote an expression *entirely distinct from those octaves*, namely one of the form

$$Q + \sqrt{-1}Q';$$

where $\sqrt{-1}$ is the old and *ordinary imaginary of algebra* (and is therefore quite *distinct* from i, j, k), while Q and Q' are abridged symbols for *two different quaternions* of the kind $w + ix + jy + kz$, introduced into analysis in 1843. *Biquaternions* of this sort have repeatedly forced themselves on the attention of Sir W. R. H., in questions respecting *geometrical impossibility, ideal intersections, imaginary deformations*, and the like.

the Cartesian Coordinates, or by some less algebraical and more purely geometrical method, the following theorems (if not already known), which have thus been *found* by the Quaternions, will doubtless be led to perceive *many* new truths, connected with them, which have escaped the present writer; although he too has arrived at other connected results, which he must suppress in the following notice.

87. I. An *ellipsoid* (E) being given, and also a system of any *even* number of points of space, $A_1, A_2, \dots A_{2m}$, of which points it is here supposed that none are situated on the surface of the ellipsoid; it is, in general, possible to *inscribe* in this ellipsoid, *two*, and *only two*, distinct and *real polygons* of $2m$ sides, $BB_1 \dots B_{2m-1}$ and $B'B'_1 \dots B'_{2m-1}$, such that the sides of each of these two polygons (B)(B') shall pass, respectively and successively, through the $2m$ given points; or in other words, so that $BA_1B_1, B_1A_2B_2, \dots B_{2m-1}A_{2m}B_{2m}$, and also $B'A_1B'_1, B'_1A_2B'_2, \dots B'_{2m-1}A_{2m}B'_{2m}$, shall be straight lines; while $B, B_1, \dots B_{2m-1}$, and also $B', B'_1, \dots B'_{2m-1}$, shall be points upon the surface of the ellipsoid.

[It should be noted that there are also, in general, what may, by the use of a known phraseology, be called *two other*, but *geometrically imaginary*, modes of inscribing a polygon, under the same conditions, in an *ellipsoid*: which modes *may become real*, by *imaginary deformation*, in passing to another surface of the second order.]

II. If we now take any *other* and *variable point* P on the ellipsoid (E) *instead* of B or B' , and make *it* the *first corner* of an inscribed polygon of $2m+1$ sides, of which the *first* $2m$ sides shall pass, respectively and successively, through the $2m$ given points (A); in such a manner that $PA_1P_1, P_1A_2P_2, \dots P_{2m-1}A_{2m}P_{2m}$, shall be straight lines, while $P, P_1, P_2, \dots P_{2m}$ shall all be points on the surface of the ellipsoid: then the *last side*, or *closing chord*, $P_{2m}P$, of this new and *variable polygon* (P), thus inscribed in the ellipsoid (E), shall *touch*, in all its positions, a certain *other ellipsoid* (E').

III. This *new ellipsoid* (E') is itself *inscribed* in the given ellipsoid (E), having *double contact* therewith, but being elsewhere interior thereto.

IV. The *two points of contact* of these two ellipsoids are the points B and B' ; that is, they are the *first corners* of the *two inscribed polygons* of $2m$ sides, (B) and (B'), which were considered in I.

[So far, the results are evidently analogous to known theorems, respecting polygons in conics; what follows is more peculiar to space.]

V. If the two ellipsoids, (E) and (E'), be cut by any plane

parallel to either of their two common tangent planes, the sections will be *two similar and similarly situated ellipses*.

[For example, if the *original* ellipsoid reduce itself to a *sphere*, then the two points of contact, B and B', become two of the four *umbilics* on the *inscribed ellipsoid*.]

VI. The closing chords PP_{2m} are also tangents to a certain series or *system of curves* (c'), not generally plane, on the surface of the inscribed ellipsoid (E'); and therefore may be arranged into a *system of developable surfaces*, (d'), of which these curves (c') are the *arêtes de rebroussement*.

VII. The same closing chords may also be arranged into a *second system of developable surfaces*, (d''), which *envelope the inscribed ellipsoid* (E') and have their *arêtes de rebroussement* (c'') all situated on a certain *other surface* (E''), which is, in its turn, enveloped by the *first set of developable surfaces* (d'); so that *the closing chords* PP_{2m} *are all tangents to a second set of curves*, (c''), *and to a second surface*, (E'').

VIII. This second surface (E'') is a *hyperboloid of two sheets*, having *double contact* with the given ellipsoid (E), and also with the inscribed ellipsoid (E'), at the points B and B'; one sheet having external contact with each ellipsoid at one of those two points, and the other at the other.

IX. If either sheet of this hyperboloid (E'') be cut by a plane parallel to either of the two common tangent planes, *the elliptic section of the sheet is similar to a parallel section of either ellipsoid, and is similarly situated therewith*.

[For example, the points of contact B and B' are two of the *umbilics of the hyperboloid* (E''), when the given surface (E) is a *sphere*.]

X. The centres of the three surfaces, (E) (E') (E''), are situated on one straight line.

XI. The two systems of developable surfaces, cut the original ellipsoid, (E), in *two new series of curves*, (F'), (F''), not generally plane, which everywhere so cross each other on (E), that at any one such point of crossing, P, *the tangents to the two curves* (F') (F'') *are parallel to two conjugate semidiameters of the surface* (E) on which the curves are contained.

[For example, if the original surface (E) be a *sphere*, then these two sets of curves (F') (F'') cross each other everywhere at *right angles*, upon that spheric surface.]

XII. *Each closing chord* PP_{2m} *is cut harmonically*, at the two points, c' , c'' , where it touches the inscribed ellipsoid (E'), and the exscribed hyperboloid (E''); or where it touches the curves (c') and (c'').

XIII. The closing chords, or *the positions of the last side of the variable polygon* (p), are not, in general, all cut perpendicu-

larly by any one common surface (notwithstanding the analogy of their arrangement, or distribution in space, in many respects, to that of the normals to a surface). In fact, the two systems of developable surfaces, (D') and (D''), are *not* generally rectangular to each other, in the arrangement *here* considered, though they *are* so for any system of normals.

XIV. *Through any given point of space, A_{2m+1} , which is at once exterior to the inscribed ellipsoid (E'), and to both sheets of the exscribed hyperboloid (E''), it is in general possible to draw two, and only two, distinct and real straight lines, $P'P'_{2m}$ and $P''P''_{2m}$, of which each shall touch at once a curve (c') on (E'), and a curve (c'') on (E''), and of which each shall coincide with one of the positions of the closing chord, PP_{2m} ; in such a manner as to be the last side of a rectilinear polygon of $2m+1$ sides, $P'P'_1P'_2 \dots P'_{2m}$, or $P''P''_1P''_2 \dots P''_{2m}$, inscribed in the given ellipsoid (E), under the condition that its sides shall pass, respectively and successively, through the $2m+1$ given points, $A_1A_2 \dots A_{2m}A_{2m+1}$. But if the last of these points were given on either of the two enveloped surfaces, (E'), (E''), the problem of such inscription would in general admit of only one distinct solution, obtained by drawing through the given point the tangent to the particular curve (c') or (c''), on which that point was situated. And if the last given point A_{2m+1} were situated within the inscribed ellipsoid (E'), or within either sheet of the exscribed hyperboloid (E''), the problem of the inscription of the polygon of $2m+1$ sides would then become geometrically impossible: though it might still be said to admit, in that case, of two imaginary modes of solution.*

[To be continued.]

XXVIII. On Colouring Matters. By EDWARD SCHUNCK*.

IN the report which I had the honour of presenting last year to the British Association on Colouring Matters, I gave the results of my investigation of the colouring matters of madder. This investigation I have continued and brought to a conclusion. The subject has however proved so extensive, the number of questions arising in regard to this valuable and extensively-used tinctorial substance being very great, that I have been unable to examine any other colouring matters very minutely.

I stated in my last report, that when finely-ground madder roots are treated with hot water, a brown liquid is obtained having a sweetish bitter taste, in which acids produce a dark brown precipitate. This precipitate I stated to consist of six substances, viz. two colouring matters, two fats, pectic acid and a bitter substance. To these I now add a seventh: it is a dark brown substance which re-

* From the Report of British Association for 1848.

mains behind when the other substances have been removed by means of boiling water and alcohol ; it is soluble in caustic alkalies with a dark brown colour, and seems to be the substance to which the colour of the dark brown precipitate is due : I consider it to be oxidized extractive matter. Concerning the method of separating the other six substances contained in the dark brown precipitate, I have nothing to add to what I said in my last report, as I have not been able to discover a shorter or better plan of separating them than that which is there described. In regard to their nature, properties and composition, which I have examined more minutely, I shall in this report give a number of additional details ; before doing so however I shall make a few observations on the subject in general. I may state, in the first place, that I have arrived at the conclusion that there is only one colouring matter contained in madder, viz. alizarine ; the other substance, which I took for a colouring matter in the first instance, and which I called rubiacine, I now consider to be no colouring matter at all, for reasons which I shall presently state. I have also reason to believe that the two substances which in my first report I called fats, are not fats, but resins ; they are coloured resins similar to many others known to chemists. Of these two resins I shall call the more easily fusible one, which dissolves in a boiling solution of perchloride or pernitrate of iron, the *alpha-resin* ; the other less easily fusible one, which forms an insoluble compound when treated with perchloride or pernitrate of iron, the *beta-resin*. The method of preparing them is the same as that which I described in my former report. After the dark brown precipitate produced in a decoction of madder by acids has been successively treated with boiling water and boiling alcohol, there remains behind a dark brown substance ; on treating this substance with caustic potash, it dissolves in great part with a dark brown colour ; on filtering there remains on the filter a mixture of peroxide of iron and sulphate of lime ; on adding a strong acid to the filtered liquid a substance in dark brown flocks is precipitated, which is thrown on a filter, washed and dried. This substance, when heated on platinum foil, burns without much flame, and leaves a considerable ash. It is easily decomposed by boiling dilute nitric acid, which converts it with an evolution of nitrous acid into a yellow flocculent substance. As it is insoluble in all menstrua except the alkalies, it may be asked, how it can be extracted from madder by means of boiling water, in which it is of itself insoluble, and whether it is not possible that it may be formed during the process of boiling by the action of the air on some substance contained in the extract. I think the latter supposition very probable, and I shall presently describe a substance of almost identical properties formed by the action of the air on xanthine, the extractive matter of madder. There can however be no doubt that the brown colour of the precipitate, which is produced by acids in a decoction of madder, is due to this substance, for the other bodies contained in it are not brown, but yellow or orange-coloured in a precipitate state. This dark brown precipitate

therefore consists of the following substances :—alizarine, rubiacine, alpha-resin, beta-resin, rubian, pectic acid, and oxidized extractive matter.

I have examined the liquid filtered from the dark brown precipitate produced by acids more minutely since making my last report. If oxalic acid be used as the precipitant, the excess of acid may afterwards be removed by chalk, without leaving any lime-salt in solution. The liquid, which had a light yellow colour, was evaporated on the sand-bath. During evaporation it gradually became brown, and left at last a thick dark brown syrup, which never became dry, however long it might be exposed to the heat of the sand-bath. On redissolving this syrup in water, a considerable quantity of a dark brown powder remained behind. On again evaporating the filtered solution on the sand-bath, an additional quantity of this powder was deposited, just as in the case of extractive matter. There can be no doubt that this powder is formed by the action of the air, assisted by heat, on some soluble substance contained in the liquid. On burning a small quantity of the brown syrup in a crucible it swelled up enormously, and gave off a quantity of empyreumatic products, which burned with a flame, leaving at last a considerable quantity of white ash ; this ash was partly soluble, partly insoluble in water. The soluble part had a strong alkaline reaction ; it consisted of a trace of lime and magnesia, and a great deal of potash, combined with carbonic, sulphuric and muriatic acids. The insoluble part consisted of carbonate of lime, carbonate of magnesia, a trace of alumina, phosphate of lime and phosphate of magnesia. The solution of the brown syrup in water had an acid reaction. It gave no precipitate or peculiar colour with a persalt of iron, and therefore contained no tannic acid. The addition of alcohol produced no precipitate or coagulate, and therefore there was no gum present. On adding muriatic or sulphuric acid to it, and then boiling, it became dark-coloured and deposited a green powder. Sugar of lead produced in the solution a dirty brown flocculent precipitate, and basic acetate of lead a still more copious precipitate. A considerable quantity of the brown syrup was dissolved in water, and basic acetate of lead was added until no more precipitate was produced. The precipitate was separated by filtration, and washed with water. The percolating liquid had a yellow colour. The excess of lead was removed from it by sulphuretted hydrogen, and the filtered liquid was evaporated over sulphuric acid, since, if evaporated by the assistance of heat, the substance contained in it was changed by the air, became brown, and deposited a brown powder. After remaining over sulphuric acid for several weeks, there was left a yellow or brownish-yellow syrup like honey, which did not become dry. This substance, though not pure (as it contained salts of lime, magnesia and potash), I conceive to be identical with Kuhlmann's xanthine and Runge's madder-yellow.

If madder contains sugar, it is evident that, provided the method of operating described above be followed, it must be contained in

the same liquid as this xanthine. I have however not been able to prove its presence by direct experiment; but I have succeeded in ascertaining indirectly that madder does in reality contain sugar of some kind by means of the following experiment. Half a hundred-weight of madder was treated with boiling water for several hours. The liquor, after being reduced by boiling to a convenient compass, was mixed with some yeast, and allowed to ferment. By distillation an alcoholic liquid was obtained, which, after a second distillation, gave $21\frac{1}{2}$ ozs. of alcohol of sp. gr. 0.925, which is equivalent to 9 ozs. of absolute alcohol. It is therefore evident that madder contains sugar of some kind or other.

The precipitate produced by basic acetate of lead in the solution of the brown syrup was decomposed with sulphuretted hydrogen. The filtered liquid was evaporated, and left after evaporation a dark brown syrup, having a strongly acid taste and reaction. The brown colour was no doubt due to xanthine in its oxidized state. After being repeatedly dissolved, and the solution being each time evaporated, a dark brown powder was deposited, just as in the case of the original solution: nevertheless the acid taste always remained. It might be supposed that this taste was due to some vegetable acid; and indeed if any such acid, or the compound of any one with the alkalies or earths, had been extracted from the madder by boiling water, it would most probably have been precipitated by the basic acetate of lead, and it would be in the liquid obtained by the decomposition of the lead precipitate that we should have to look for any such acid. Now the syrup obtained after decomposing the lead precipitate and evaporating the liquid, though intensely acid, contained no oxalic, tartaric, malic or citric acid; neither did it show the least sign of crystallization; but the watery solution gave a crystalline precipitate with ammonia and sulphate of magnesia; and after destroying the brown organic matter contained in it by adding nitric acid and boiling, and then evaporating to drive away the excess of nitric acid, it gave a yellow precipitate with nitrate of silver and ammonia. I therefore infer that the acid to which the sour taste of the brown syrup was owing, was phosphoric acid*. The sulphuret of lead, produced by the decomposition of the lead precipitate, was treated with boiling caustic potash. A dark brown solution resulted, which after filtration gave with muriatic acid a dark brown precipitate. This precipitate, after filtration, washing and drying, cohered into masses, which were brittle and black, but became brown when powdered. It was totally insoluble in boiling water and alcohol. It was decomposed by dilute boiling nitric acid,

* On one occasion, after having added nitric acid to the acid syrup and boiled, I obtained on evaporation crystals of an organic acid, very similar to alizaric acid, but not identical with it. It was sparingly soluble in cold water, but very soluble in hot. It was volatile. The watery solution gave with acetate of lead a crystalline precipitate soluble in boiling water, with perchloride of iron a cream-coloured precipitate, with acetate of copper a green crystalline precipitate, and with nitrate of silver and ammonia a white flocculent precipitate. Alizarate of lead is quite insoluble in boiling water, and not in the least crystalline.

and changed into a yellow flocculent substance. It was soluble in concentrated sulphuric acid, forming a brown liquid, and was reprecipitated by water. I consider this substance, that formed in a solution of xanthine during evaporation by heat, and the dark brown substance contained in the precipitate produced by acids in a decoction of madder as the same, and that they are all produced from xanthine by the action of the oxygen of the air.

It still remains for me to say a few words on the substances left behind in the root, after madder has been exhausted with boiling water. It has for some time been well known that if madder, which has already been used for the purpose of dyeing, be treated with a strong acid such as sulphuric or muriatic, and the acid be then carefully removed by washing with cold water, it is capable of being again used for dyeing in the same way as fresh madder. It is in this manner that the article known in commerce as *garanceux* is manufactured. This is a convincing proof that it is impossible to extract the whole of the colouring matter by means of boiling water, and that part of it must remain behind in some state in which it is insoluble in water. A quantity of madder was treated with boiling water until the liquor gave absolutely no more precipitate on the addition of muriatic acid. A very long boiling was necessary for this purpose. The colour of the madder was changed by this process from yellowish-brown, as it appears in the fresh state, to a dull red. It was then treated with boiling caustic potash ley. A liquor of a brownish colour was obtained, in which muriatic acid produced a gelatinous precipitate of a brown colour. This was separated by filtration, and, after being washed with cold water in order to remove all the muriatic acid, was treated with a large quantity of boiling water, in which it proved to be almost entirely soluble. The solution was light brown. It gave gelatinous precipitates with acids, with lime and baryta water, alcohol and most salts. On evaporation it left a substance in light brown, transparent, brittle scales, which turned out to be pectic acid, much purer indeed than that obtained in the first instance from the aqueous decoction. No colouring matter, or any other substance besides pectic acid, seemed to be extracted by the caustic alkali.

Another quantity of madder which had been completely exhausted by boiling water, was treated with boiling muriatic acid, and the liquid, after the boiling had been kept up for some time, was strained through a cloth and supersaturated with ammonia, which produced a pinkish-white precipitate. This precipitate was thrown on a filter and carefully washed. The liquid contained an abundance of lime and magnesia. A part of the pinkish-white precipitate was dried and heated to redness in a crucible. During ignition a gas came off which was without odour, and burnt with a blue flame, being probably carbonic oxide. After complete ignition it dissolved in muriatic acid with an effervescence of carbonic acid, but without leaving much carbonaceous residue. On adding ammonia to the solution a white precipitate was again produced. The filtered liquid

contained a large quantity of lime and a trace of magnesia. The precipitate consisted of alumina, peroxide of iron, phosphate of lime, and a trace of phosphate of magnesia. As it became probable from the preceding reactions that the pinkish-white precipitate contained oxalate of lime, the rest of it was treated with boiling dilute sulphuric acid. The liquid after filtration was evaporated. It gave crystals which were dissolved in alcohol to separate the sulphate of lime. The alcohol on evaporation gave colourless crystals of pure oxalic acid. Hence I infer that the following substances were extracted from the madder by means of muriatic acid :—lime, magnesia, oxalate of lime, phosphate of lime, alumina and peroxide of iron. The madder which had been subjected to the action of muriatic acid was now well-washed with water, and then treated with boiling caustic potash ley. A dark red solution was obtained, which, after being strained through a cloth, produced, on being supersaturated with an acid, a dark reddish-brown precipitate. This precipitate was thrown on a filter, and well-washed with cold water, to remove the excess of acid. I found this precipitate to dye mordanted cloth quite full, and of the same colours as madder itself. There could therefore be no doubt about its containing alizarine. Moreover on treating the precipitate with boiling alcohol, a brownish-yellow liquid was obtained, which left on evaporation a brownish-red residue. A small portion of this residue being heated between two watch-glasses, an abundance of orange-coloured crystals of sublimed alizarine appeared on the upper glass. By treating the precipitate with boiling water, and filtering boiling hot, the liquid deposited on cooling orange-coloured flocks, which were impure alizarine, for they dyed mordanted cloth, and after being dried and heated in a tube, they gave a crystalline sublimate. The liquid gave on evaporation pectic acid. That part of the precipitate which was left undissolved by boiling water, was treated with a boiling solution of nitrate of iron. The filtered liquid gave, on the addition of muriatic acid, a slight yellow precipitate, which was probably rubiacic acid from the rubiacine of the precipitate. The greater part was insoluble in nitrate of iron. By treating the insoluble residue with boiling muriatic acid, filtering, washing with water, and treating with boiling alcohol, an abundance of beta-resin was procured.

I infer from these experiments that the substances extracted from madder by caustic potash, after exhaustion with boiling water and treating with acid, previously existed in the root in combination with lime and magnesia; that these substances are not different from those extracted by boiling water, viz. alizarine, rubiacine, resins and pectic acid; that the compounds of these bodies with lime and magnesia are insoluble in water, and, with the exception of pectate of lime, insoluble in caustic alkalies; and that therefore, in order to extract them by means of water or an alkali, it is first necessary to remove the lime and magnesia with which they are combined by means of an acid.

I shall now proceed to give some further details concerning the properties and composition of the substances extracted from madder.

Alizarine.—Concerning the properties of alizarine I have nothing to add to what I stated in my last report, except that when crystallized from alcohol it contains several atoms of water of crystallization, which it loses when heated to 212° F. The crystals after being heated to this point have not lost their shape, but have become opaque and of a much redder colour, resembling that of native chromate of lead. On placing them in a tube immersed in a sulphuric-acid bath, and heating the bath, no further change takes place until about 420° F., when a sublimate of orange-coloured crystals begins to appear on the cold part of the tube.

On subjecting alizarine to elementary analysis I obtained the following results:—

I. 0.3205 grm. of crystallized alizarine dried in the air gave, on being burnt with chromate of lead, 0.6695 carbonic acid and 0.1210 water.

II. 0.3985 grm. of the same gave 0.8320 carbonic acid and 0.1850 water.

III. 0.3140 grm. gave 0.6565 carbonic acid and 0.1670 water.

These numbers correspond in 100 parts to—

	I.	II.	III.
Carbon	56.97	56.94	57.02
Hydrogen	4.19	5.13	5.87
Oxygen	38.84	37.93	37.11
	100.00	100.00	100.00

The great discrepancy in the amounts of hydrogen in the preceding analyses arises from the circumstance that alizarine loses its water of crystallization with such extreme facility. No. I. was mixed with warm chromate of lead in a warm mortar; No. II. was mixed with warm chromate of lead in a cold mortar; and No. III. with cold chromate of lead in a cold mortar. In the case of No. I. therefore we see that the heat of the chromate of lead and the mortar combined was sufficient to drive away more water than what corresponds to $1\frac{1}{2}$ per cent. of hydrogen, though this heat was not greater than what might be borne by the hand. In order to determine the amount of water of crystallization, crystallized alizarine was heated in a water-bath until it lost no more in weight.

I. 0.4015 grm. treated in this way lost 0.0735 water.

II. 0.3575 grm. lost 0.0655 water.

Alizarine which had been deprived of its water of crystallization by heat, gave, on being burnt with chromate of lead, the following results:—

I. 0.2990 grm. gave 0.7575 carbonic acid and 0.1045 water.

II. 0.3005 grm. of a different preparation gave 0.7620 carbonic acid and 0.1095 water.

III. 0.2765 grm. of the same preparation as the preceding gave 0.7010 carbonic acid and 0.1025 water.

In 100 parts it contains therefore—

	I.	II.	III.
Carbon	69.09	69.15	69.14
Hydrogen	3.88	4.04	4.11
Oxygen	27.03	26.81	26.75
	100.00	100.00	100.00

On analysing alizarine prepared by sublimation from pure crystals, I obtained the following numbers:—

I. 0·3970 grm. gave 1·0115 carbonic acid and 0·1340 water.

II. 0·4110 grm. gave 1·0510 carbonic acid and 0·1375 water.

In 100 parts—

	I.	II.
Carbon	69·48	69·73
Hydrogen	3·75	3·71
Oxygen	26·77	26·56
	100·00	100·00

It will be seen from this that sublimed alizarine does not differ materially in composition from alizarine which has been freed from its water of crystallization.

Of the compounds of alizarine with bases I prepared the lime, baryta and lead compounds. The two former were prepared by dissolving alizarine in ammonia, and precipitating with chloride of calcium and chloride of barium, the latter by dissolving alizarine in alcohol and precipitating with an alcoholic solution of sugar of lead. The latter forms a purple precipitate, which, after standing for some hours, becomes of a dull red.

The lead compound gave on analysis the following numbers:—

I. 0·4800 grm. gave 0·2095 oxide of lead and 0·0245 metallic lead, equivalent to 0·2359 oxide of lead.

0·5125 grm. burnt with chromate of lead gave 0·7050 carbonic acid and 0·0780 water.

II. 0·5865 grm. of a different preparation gave 0·3970 sulphate of lead, equivalent to 0·2920 oxide of lead.

0·6915 grm. gave 0·9370 carbonic acid and 0·1005 water. Hence was deduced the following composition:—

	Calculated Numbers.	Found.	
		I.	II.
14 eqs. Carbon.....	84	37·57	37·51
4 „ Hydrogen ..	4	1·78	1·67
3 „ Oxygen	24	10·75	11·70
1 „ Oxide of lead	111·7	49·90	49·12
	223·7	100·00	100·00

The lime compound gave the following results:—

I. 0·4685 grm. gave 0·2065 sulphate of lime, equivalent to 0·0857 lime.

II. 0·4750 grm. gave 0·2125 sulphate of lime, equivalent to 0·0882 lime.

Assuming that the formula for this compound is $C_{14}H_4O_3 + CaO + HO$, its composition would be as follows:—

	Calculated Numbers.	Found.	
		I.	II.
1 eq. Alizarine.....	112	74·91	
1 „ Water	9	6·03	
1 „ Lime	28·5	18·30	18·58
	149·5	100·00	

The baryta compound gave the following :—

0·2450 grm. gave 0·1420 sulphate of baryta, equivalent to 0·0932 baryta.

Assuming that the formula of this compound is similar to that of the last, viz. $C_{14} H_4 O_3 + BaO + HO$, its composition would be as follows :—

		Calculated.	Found.
1 eq. Alizarine.....	112	56·65	
1 „ Water.....	9	4·57	
1 „ Baryta	76·68	38·78	38·03
	197·68	100·00	

Neither of these compounds loses the equivalent of water which it contains on being heated in a water-bath for several hours.

The composition of crystallized alizarine must therefore be as follows :—

		Calculated.
14 eqs. Carbon	84	56·75
8 „ Hydrogen	8	5·40
7 „ Oxygen	56	37·85
	148	100·00

or,

		Calculated Numbers.	Found*.	
			I.	II.
1 eq. dry Alizarine....	121	81·76		
3 eqs. Water	27	18·24	18·33	18·32
	148	100·00		

It follows that alizarine dried at 212° must consist of—

		Calculated.
14 eqs. Carbon	84	69·42
5 „ Hydrogen	5	4·13
4 „ Oxygen	32	26·45
	121	100·00

If this be the true composition of alizarine, it follows that there exists a very singular relation between it and the composition of benzoic acid. The formula of benzoic acid is $C_{14} H_6 O_4$, and alizarine only differs from it therefore by containing one equivalent less of hydrogen. If we compare alizarine with isatine, we shall find that the latter only differs from the former by containing in addition the elements of one equivalent of cyanogen. The formula of isatine is $C_{16} H_5 NO_4 = C_{14} H_5 O_4 + C_2 N$. Anthranilic acid differs in composition from alizarine in containing in addition the elements of amidogene, for the formula of anthranilic acid is $C_{14} H_7 NO_4 = C_{14} H_5 O_4 + NH_2$.

Alizaric Acid.—In my former report I stated that alizarine, when treated with concentrated solutions of persalts of iron, is converted into a new acid, which I called alizaric acid. I stated at the same

* See pp. 210, 211.

time that I thought it probable that alizaric acid might also be formed by acting on alizarine with nitric acid. This supposition has since been confirmed. On treating pure crystallized alizarine with boiling nitric acid, it is decomposed with an evolution of nitrous acid, and the liquid on evaporation gives crystals of alizaric acid. It is however not necessary to prepare pure alizarine in order to obtain alizaric acid. I have found the following to be the easiest method:—Nitric acid of about sp. gr. 1.20 having been put into a retort, garancine is introduced into the acid, and the liquid is heated until the red fumes have ceased to be evolved, and the colour of the garancine has changed from dark brown to yellow. The reddish-yellow acid liquid which is obtained, is filtered or strained to separate it from the woody fibre, &c. of the garancine, and evaporated to crystallization. A yellow crystalline mass is obtained, which is a mixture of oxalic acid and impure alizaric acid. After being washed with cold water to remove the excess of nitric acid, the mass is dissolved in boiling water, and chalk is added until all effervescence and acid reaction have ceased. The liquid is filtered, and the oxalate of lime remaining on the filter is washed with boiling water, until no more lime can be detected in the percolating liquid. The liquid is a solution of alizarate of lime. Muriatic acid is added to it, and it is evaporated to crystallization. A yellow mass is again obtained, which may be washed with cold water to remove the chloride of calcium, then redissolved in boiling water. It forms a yellow solution, which may be almost decolorized by animal charcoal. On again evaporating, the alizaric acid is obtained in large crystals. Should these crystals still retain a yellow tinge, which is generally the case, they must be redissolved in boiling water. By passing chlorine gas through the boiling solution, until every trace of colour has disappeared, perfectly colourless crystals of the acid are obtained on cooling. Prepared in this way, it appears in large flat rhombic plates: it has the properties which I described in my last report.

The salts of alizaric acid are mostly soluble. Alizarate of potash is formed by neutralizing a watery solution of alizaric acid with carbonate of potash: it is obtained on evaporation as a deliquescent mass. Alizarate of lime is prepared by neutralizing alizaric acid with carbonate of lime, and evaporating to crystallization. It crystallizes in prisms, possessing great lustre. Alizarate of baryta, prepared in the same way by means of carbonate of baryta, crystallizes in silky needles. Alizarate of silver, prepared by double decomposition, is soluble in boiling water, from which it crystallizes on the solution cooling. Alizarate of lead is an insoluble white powder, obtained by precipitation of the acid with sugar of lead. With ammonia alizaric acid does not seem to form a neutral salt. On supersaturating a solution of the acid with ammonia and evaporating, the solution acquires during evaporation an acid reaction, and at length a salt crystallizes out in flat plates, which is no doubt a superalizarate of ammonia. All the salts of alizaric acid, when strongly heated, are decomposed with an evolution of a fragrant smell similar

to that of benzene, and give, as a product of the decomposition, a thick brown oil, to which without doubt the smell is owing; while the carbonates of the bases, or the bases themselves, remain behind mixed with much charcoal.

The elementary analysis of alizaric acid gave the following results:—

I. 0.5250 grm. obtained by means of perchloride of iron and burnt with oxide of copper, gave 1.1015 carbonic acid and 0.1810 water.

II. 0.4670 grm. obtained by means of nitric acid and burnt with chromate of lead, gave 0.9865 carbonic acid and 0.1685 water.

III. 0.4475 grm. of the same preparation as the preceding gave 0.9360 carbonic acid and 0.1625 water.

IV. 0.4395 grm., purified by means of chlorine and burnt with chromate of lead, gave 0.9335 carbonic acid and 0.1510 water.

These numbers give in 100 parts—

	I.	II.	III.	IV.
Carbon	57.20	57.61	57.10	57.92
Hydrogen	3.83	4.00	4.03	3.81
Oxygen	38.97	38.39	38.87	38.27
	100.00	100.00	100.00	100.00

Alizarate of lead was analysed with the following results:—

I. 0.8110 grm. gave 0.2665 oxide of lead and 0.2160 metallic lead, equivalent to 0.4991 oxide of lead.

0.6660 grm. gave 0.5810 carbonic acid and 0.0915 water.

II. 0.6230 grm. gave 0.2040 oxide of lead and 0.1655 metallic lead, equivalent to 0.3822 oxide of lead.

0.6515 grm. gave 0.5560 carbonic acid and 0.0860 water. Hence was deduced the following composition:—

			Found.	
			I.	II.
14 eqs. Carbon	84	23.37	23.79	23.27
4 „ Hydrogen ..	4	1.11	1.52	1.46
6 „ Oxygen	48	13.37	13.15	13.93
2 „ Oxide of lead	223.4	62.15	61.54	61.34
	359.4	100.00	100.00	100.00

The baryta salt lost nothing in weight on being heated for several hours in a water-bath.

I. 0.6725 grm. of baryta salt dried at 212° gave 0.5245 sulphate of baryta, equivalent to 0.3442 baryta.

II. 0.7330 grm. gave 0.5700 sulphate of baryta, equivalent to 0.3740 baryta.

Its composition is therefore probably as follows:—

		Calculated.	Found.	
			I.	II.
1 eq. anhydrous Acid .	136	46.26		
1 „ Water	9	2.36		
2 eqs. Baryta	153.3	51.38	51.18	51.03
	<u>298.3</u>	<u>100.00</u>		

It is probable that the silver salt also contains two equivalents of base to one of acid.

It follows from the analysis of the lead salt, that the hydrated acid has the following composition:—

		Calculated.
14 eqs. Carbon	84	57.93
5 „ Hydrogen	5	3.44
7 „ Oxygen	56	38.63
	145	100.00

By the action of nitric acid on alizarine the latter takes up three equivalents of oxygen without losing any hydrogen, for $C_{14}H_5O^4 + 3O = C_{14}H_5O_7$. It appears also that alizaric acid contains one equivalent of hydrogen less, and three equivalents of oxygen more, than benzoic acid.

Pyro-alizaric Acid.—When alizaric acid is heated it is totally volatilized, and forms a sublimate in the shape of long white needles, to which I have given the name of pyro-alizaric acid. By the action of heat alizaric acid loses water, or the elements of water. Pyro-alizaric acid is soluble in boiling water. The solution, however, produces exactly the same reactions as alizaric acid itself, and on evaporation large rhombic crystals are obtained, which have quite the appearance of the latter acid. It is probable therefore that, by solution in water, pyro-alizaric acid takes up again the elements of water, and is reconverted into alizaric acid. The following results were obtained on analysing this acid:—

I. 0.4405 grm. dried at 212° and burnt with chromate of lead, gave 1.0345 carbonic acid and 0.1185 water.

II. 0.4255 grm. gave 0.9985 carbonic acid and 0.1215 water.

From these numbers it may be inferred that the composition is as follows:—

		Calculated Numbers.	Found.	
			I.	II.
28 eqs. Carbon	168	63.87	64.04	63.99
7 „ Hydrogen	7	2.66	2.98	3.17
11 „ Oxygen	88	33.47	32.98	32.84
	263	100.00	100.00	100.00

Hence it follows that by the action of heat two equivalents of alizaric acid lose three equivalents of water, and give one equivalent of pyro-alizaric acid, since $2(C_{14}H_5O_7) - 3HO = C_{28}H_7O_{11}$.

Rubiaccine.—In my last report I described the method of preparation, and the properties of rubiacine and rubiacic acid, and I have nothing further to add to what I there stated. I may mention however that I have arrived at the conclusion that rubiacine cannot be considered as a true colouring matter, as it is impossible to dye with it. I shall also show that, contrary to the opinion which I was led to entertain in the first instance, rubiacine does not contribute to produce any effect in the process of madder-dyeing.

On subjecting rubiaccate of potash and rubiacic acid to analysis, I obtained the following results:—

I. 0·4490 grm. rubiacate of potash gave 0·1090 sulphate of potash, equivalent to 0·0589 potash.

0·4350 grm. gave 0·7950 carbonic acid and 0·0900 water.

II. 0·3245 grm. gave 0·0790 sulphate of potash, equivalent to 0·0427 potash.

0·2890 grm. gave 0·5315 carbonic acid and 0·0665 water.

From these numbers it may be inferred that the salt is composed as follows:—

		Calculated Numbers.	Found.	
			I.	II.
31 eqs. Carbon....	186	51·63	51·50	51·82
7 „ Hydrogen..	7	1·94	2·29	2·55
15 „ Oxygen ..	120	33·31	33·09	32·47
1 „ Potash	47·27	13·12	13·12	13·16
<hr/>		<hr/>	<hr/>	<hr/>
	360·27	100·00	100·00	100·00

I. 0·3785 grm. rubiacic acid, dried at 212° and burnt with oxide of copper, gave 0·7940 carbonic acid and 0·0845 water.

II. 0·3605 grm. of another preparation gave 0·7610 carbonic acid and 0·0795 water.

III. 0·4670 grm. of the same preparation as the preceding gave 0·9775 carbonic acid and 0·1050 water.

Hence was deduced the following composition:—

		Calculated Numbers.	Found.		
			I.	II.	III.
31 eqs. Carbon ..	186	57·76	57·21	57·57	57·08
8 „ Hydrogen	8	2·48	2·48	2·45	2·49
16 „ Oxygen ..	128	39·76	40·31	39·98	40·43
		322	100·00	100·00	100·00

0·3150 grm. rubiacine, dried at 212° and burnt with oxide of copper, gave 0·7740 carbonic acid and 0·0935 water.

This gives the following composition:—

		Calculated.	Found.
31 eqs. Carbon.....	186	67·63	67·01
9 „ Hydrogen	9	3·27	3·28
10 „ Oxygen	80	29·10	29·71
		275	100·00

The formula of rubiacine being $C_{31}H_9O_{10}$, and that of rubiacic acid $C_{31}H_8O_{16}$, it follows that when rubiacine is converted into rubiacic acid, it loses one equivalent of hydrogen and takes up six equivalents of oxygen, and that when rubiacic acid is reconverted into rubiacine, it loses six equivalents of oxygen and takes up again one of hydrogen. This oxidation and reduction is accomplished with the same certainty and precision as any similar process with inorganic bodies.

Alpha-resin.—This resin is a constituent of the dark brown precipitate produced by acids in a decoction of madder. It dissolves together with rubiacine, when this precipitate is treated with a boil-

ing solution of perchloride or perntrate of iron, and is precipitated together with rubiacine and rubiacic acid when muriatic acid is added to the solution. It is separated from the rubiacine and rubiacic acid by means of alcohol, in which it is easily soluble, while the two former are but little soluble. It has a dark brown or reddish-brown colour. When cold it is brittle, and may be easily pulverized. It begins to become soft at 150° F., and melts to dark brown drops between 200° and 212° . When heated on platinum-foil it melts, swells up, and burns with flame, leaving much charcoal, which however burns away without leaving any residue. When heated in a glass tube it swells up, gives an oily sublimate, and evolves a strong smell, leaving at last a bulky carbonaceous residue. It is slightly soluble in boiling water, to which it communicates a yellow tinge. On the solution cooling yellow flocks are deposited, which are increased in quantity by adding an acid. It dissolves in alcohol with an orange colour; water makes the solution milky, and on the addition of an acid the resin is completely precipitated in orange-coloured flocks. The alcoholic solution does not redden litmus paper. It dissolves in concentrated sulphuric acid with a dark orange colour, and is reprecipitated by water in yellow flocks. It is decomposed by boiling concentrated nitric acid; on evaporating the acid a resinous mass is left. It dissolves in caustic and carbonated alkalies with a purplish red colour. The solution in ammonia does not lose its ammonia on boiling, but on evaporation the resin is left in combination with a little ammonia. The ammoniacal solution gives purple precipitates with the chlorides of barium and calcium, and a dirty red precipitate with alum. It dissolves in perchloride and perntrate of iron with a dark reddish-brown colour, and is re-precipitated by acids in flocks. The alcoholic solution gives red precipitates with alcoholic solutions of sugar of lead and acetate of copper. If chlorine be passed through a solution of the resin in caustic potash, it is decolorized; acids however now produce no precipitate, so that the resin seems to have been entirely decomposed by the chlorine. If mordanted cloth be introduced into boiling water, in which a quantity of the resin is suspended, the alumina mordant acquires an orange colour, and the iron mordant a brown colour. Nevertheless these colours are so slight, that it is not likely that this resin contributes in any way to produce the desired effect in the process of madder-dyeing. I shall presently show that, on the contrary, it is rather injurious than otherwise in this process, since those parts of the cloth which should remain white acquire from it a disagreeable yellow tinge, which cannot afterwards be removed by merely washing with water, so that even if it did contribute to produce any greater intensity of colour on the mordanted parts, the advantage would be more than counterbalanced by the injurious effect on the unmordanted parts.

Beta-resin.—This resin also forms a constituent of the dark brown precipitate produced by acids in a decoction of madder. If this precipitate be treated with a boiling solution of perchloride or per-

nitrate of iron, the beta-resin forms a compound with peroxide of iron, which remains undissolved. By decomposing this compound with muriatic acid, and dissolving the resin in boiling alcohol, it is deposited on the alcohol cooling as a light brown powder. It hardly melts at the temperature of boiling water, but merely becomes soft and coheres into lumps. When heated on platinum foil, it melts and burns, leaving a slight red ash. When heated in a glass tube, it gives yellow fumes and evolves a disagreeable smell, leaving a carbonaceous residue. It is slightly soluble in boiling water, to which it communicates a yellow tinge; on the solution cooling nothing separates, but on adding acid some yellow flocks are deposited, while the liquid becomes colourless. The alcoholic solution is dark yellow; it reddens litmus-paper. Water renders it milky, and acids precipitate the resin completely in yellow flocks. The resin dissolves in concentrated sulphuric acid with a dark brown colour, and is re-precipitated by water in light brown flocks. Concentrated nitric acid dissolves it on boiling and decomposes it; on evaporation there is left a yellow, bitter astringent substance. It dissolves in caustic and carbonated alkalis with a dirty red colour, inclining to purple in the case of caustic alkali. It is re-precipitated by acids in brown flocks. If chlorine be passed through a solution of the resin in caustic potash, it is decolorized; but the substance itself seems to be thereby decomposed, as acids afterwards produce only a slight precipitate. The ammoniacal solution gives with the chlorides of barium and calcium dirty yellow precipitates. The alcoholic solution gives with an alcoholic solution of sugar of lead a red precipitate, and with an alcoholic solution of acetate of copper a brown precipitate. The ammoniacal solution loses its ammonia on evaporation, and the resin is left as a transparent brown skin. This resin has the same effect on mordanted cloth as the preceding; the alumina mordant acquires an orange, and the iron mordant a brown colour, while the unmordanted parts become yellow and unsightly. These effects are not however so decided as in the case of the alpha-resin, which is probably owing to its being less soluble in water than the latter.

Rubian.—I have given this name to the substance to which the bitter taste of madder seems to be due. I have described its method of preparation and properties in my last report. I may state, in addition to what I there said, that rubian seems to be a nitrogenous body, since, on treating it with boiling caustic alkali, ammonia is evolved. This fact and the bitter taste seem to indicate that the medical properties of madder, if indeed it possesses any, reside in this substance.

If a solution of rubian in water be evaporated in contact with the air and with the assistance of heat, it deposits a dark brown substance, which sinks to the bottom in resinous drops, so that on treating the residue after evaporation with water, it is not completely redissolved; and if the filtered liquid be again evaporated as before, a fresh quantity of the dark brown substance is formed, just as in the case of extractive matter. This dark brown substance melts

into drops in boiling water, but when cold it is brittle. It dissolves in alkalies with a dark red colour, and is re-precipitated by acids in yellow flocks; indeed it bears in all respects a great resemblance to the body which I have called alpha-resin. Nevertheless it seems to consist of more than one substance; for if it be heated in a glass tube over a lamp, an abundant sublimate, consisting of shining yellow crystals, is obtained in the upper part of the tube: these crystals very much resemble rubiacine. If it be treated with a boiling solution of perchloride or pernitrate of iron, the liquid becomes reddish brown, and gives after filtration a yellow precipitate with muriatic acid, which is a proof of its containing either alpha-resin or rubiacine, or both. Hence it becomes very probable that rubiacine, the alpha-resin, and perhaps also the beta-resin, are formed from rubian by the action of the oxygen of the air. It becomes still more probable when we consider the following facts:—If an infusion of madder with cold water be allowed to stand in contact with the air, it will be found that after some hours the liquid is filled with a number of long hair-like crystals, which are, as I have shown on a previous occasion*, rubiacine, generally mixed with a substance having all the properties of beta-resin. I have had one specimen of madder which gave such quantities of rubiacine on allowing the infusion to stand, that it collected on the surface of the liquor as a bright yellow scum, and by crystallizing it from alcohol it was obtained almost in a state of purity. Now as rubiacine is insoluble in cold water, it must in this case either have been formed from some substance contained in the infusion by the action of the air, or else it was at first held in solution by some other substance, such as an alkali or alkaline earth from which it gradually became separated, as by the formation of some acid in the liquid. I incline to the former supposition, and think it probable that it is the rubian which by its oxidation gives rise to the rubiacine.

Xanthine.—This substance, the method of preparing which from a decoction of madder after the separation of the colouring matters, &c. by acid, I have described above, is of course not a pure substance, since after ignition it leaves a considerable quantity of fixed residue: it is also probable that it contains a small quantity of sugar, as I stated before. Nevertheless it produces reactions of a peculiar kind, which cannot be attributed to sugar, gum, or any similar substance, and can only be due to a peculiar body which exists only in madder. It has the following properties:—When prepared as above described, it is a thick, viscid, yellow or brownish-yellow syrup, resembling honey in colour and consistency, which cannot be rendered dry even by exposing it to a heat at which it begins to be decomposed. When exposed to the air, it becomes more liquid on account of its attracting moisture. When heated to ignition, it swells up enormously, giving off at the same time a very perceptible smell of aceton and burns, leaving at last a considerable quantity of ash,

* See the Report of the British Association for the Advancement of Science for 1846.

which consists of the carbonates of lime, magnesia and potash. It is without doubt the acetates of those bases which, being mixed with the substance, produce the smell of acetone during ignition. The acetic acid was of course derived from the basic acetate of lead used in the preparation of xanthine, and the acid with which they were originally combined must have gone to the oxide of lead. Now, as I stated above, the oxide of lead was found to be combined with phosphoric acid; hence it is probable that the greater part, if not all, of the fixed bases left after the ignition of the xanthine existed in the plant as phosphates. Xanthine has a disagreeable taste, between bitter and sweet. The watery solution is yellow. It is soluble in alcohol, and is left after evaporation in the same state as before. It is insoluble in æther. On adding muriatic or sulphuric acid to the watery solution and boiling for some time, a peculiar smell is evolved, the solution becomes gradually dark green, and a dark green powder is deposited. This is the most characteristic property of xanthine. Nitric acid does not produce the same dark green powder, or any deposit on boiling; nevertheless the powder which has once been formed by means of muriatic or sulphuric acid, is not dissolved by boiling nitric acid, but only turned yellow. Acetic acid produces no effect. Oxalic acid gives a white precipitate of oxalate of lime. Bichromate of potash and sulphuric acid produce no effect on a solution of xanthine, even on boiling. On adding caustic potash to the solution it turns brown, and on boiling a slight smell of ammonia is evolved. Lime and baryta water, acetate and basic acetate of lead, the acetates of alumina, iron and copper, nitrate of silver, corrosive sublimate, and a solution of glue, produce no precipitate or effect whatever in a solution of xanthine. In fact it does not seem to be precipitated by any reagent whatever without undergoing decomposition.

If a clear light yellow watery solution of xanthine be evaporated with the assistance of heat and in contact with the air, as on the sand-bath, to a syrup, and this syrup be again mixed with water and the solution again evaporated, the process being several times repeated, the solution gradually becomes dark brown, and at length a dark brown powder is deposited. The brown solution now gives with acetate, or basic acetate of lead, a thick brown precipitate. The filtered liquid is yellow, and if the excess of lead be removed by sulphuretted hydrogen, the solution again gives, on evaporation over sulphuric acid, a colourless or light yellow syrup, which however, if redissolved and evaporated with the assistance of heat as before, again becomes dark brown, and deposits a dark brown powder. There can therefore be no doubt that this brown powder is a product of the oxidation of xanthine, that xanthine is a species of extractive matter, and that the brown powder stands in the relation to it of an *apothema*. This brown powder has the following properties:—When dry it is a dark brown mass, easily reduced to powder. It is quite insoluble in boiling water and boiling alcohol. It burns without flame, leaving much ash. It is soluble in concentrated

sulphuric acid with a dark brown colour, and is re-precipitated by water. Boiling dilute nitric acid decomposes it with an evolution of nitrous acid, and changes it into a yellowish-red flocculent substance. Concentrated nitric acid on boiling decomposes and dissolves it entirely. It dissolves in caustic and carbonated alkalies with a dark brown colour, and is re-precipitated by acids in light brown flocks. The ammoniacal solution gives brown precipitates with the chlorides of barium and calcium. The dark green powder which is produced by the action of sulphuric and muriatic acid on xanthine, has the following properties:—When dry it has a dark olive colour. It burns with a flame and a smell like burning wood, leaving a large quantity of charcoal, which however burns away without any fixed residue. It is decomposed by boiling dilute nitric acid, and changed into a yellow flocculent substance. It is insoluble in concentrated sulphuric acid, and also in boiling alcohol. When treated with caustic potash, a part dissolves with a dark brown colour, and is re-precipitated by acids as a dark brown powder, while the other part remains undissolved as a black powder.

Mordanted cloth acquires no colour in a boiling solution of xanthine, if the latter is in its yellow unoxidized state; but if the solution has become brown by contact with the air, then both the alumina and the iron mordant acquire in the boiling solution a brown colour, while the unmordanted parts, which should remain white, become of a brown tint. Hence it follows that xanthine is injurious in madder-dyeing, and must contribute, together with the two resins, in impairing the purity of the colours, and sullyng the whiteness of those parts which should attract no colour. To get rid of the xanthine is one object of changing madder into garancine.

It remains for me to say a few words in regard to the part which the different substances described above play in the process of madder-dyeing. I regret to say that in my last report there are contained some views on this head, which I have found, on more exact investigation, to be erroneous. The two principal points to be determined are, which is the substance that produces the chief effect in dyeing with madder, and why is a certain proportion of lime, either in the plant or in the dye-bath, necessary for the production of fine and durable colours. In my last report I stated it as my opinion, that both alizarine and rubiacine take part in the process, that rubiacine alone produces no effect, but that when it is in combination with an alkali or an alkaline earth, it forms double compounds with the alizarine compounds of alumina and peroxide of iron, and thus increases the intensity of colour in the latter. I have since found that this opinion cannot be sustained, since rubiacine, whether free or combined, produces no beneficial effect in the process of dyeing, and is therefore no true colouring matter, as the following experiments will show.

Since the brown precipitate produced by acids in a watery ex-

tract of madder contains all the free colouring matter of the root, and acts in dyeing in the same way as madder itself, it was evident that by trying the constituents of this precipitate in conjunction with one another, both in a free state and in combination with lime, a correct view of the part performed by each would be arrived at. Having therefore taken a piece of calico on which three mordants had been printed, one for red, one for purple, and one for black, in alternate stripes, each stripe being one quarter of an inch broad, and having intervals between them of the same width, it was divided into pieces of six inches by three, and one of these pieces was taken for each of the following experiments. As the tinctorial power of alizarine is very great, so great that one quarter of a grain was enough to over-dye one of these pieces, I took one or two grains of crystallized alizarine, dissolved it in a measured quantity of water, to which a little caustic alkali had been added, and was then able to divide the solution into portions corresponding to quarters, eighths, and sixteenths of a grain, so that by precipitating one of these portions with muriatic acid, filtering and carefully washing, I obtained small quantities in a state very well adapted for dyeing. By treating one of these quantities while on the filter with lime-water, and washing out the excess of lime, I obtained small quantities of the lime compound of alizarine for the same purpose. The same process was used for obtaining small quantities of rubiacine, alpha-resin, beta-resin, pectic acid and rubian, and their lime compounds. Each experiment was performed with the same quantity of water, at as nearly as possible the same temperature, and occupied the same length of time, viz. half an hour. The substances used, and their quantities, were as follows :—

1. $\frac{1}{8}$ grain of alizarine.
2. $\frac{1}{16}$ gr. alizarine.
3. $\frac{1}{16}$ gr. alizarine and $\frac{1}{16}$ gr. alizarine in combination with lime.
4. $\frac{3}{32}$ gr. alizarine and $\frac{1}{32}$ gr. alizarine in combination with lime.
5. $\frac{1}{8}$ gr. alizarine and $\frac{1}{8}$ gr. rubiacine.
6. $\frac{1}{8}$ gr. alizarine and $\frac{1}{8}$ gr. rubiacine in combination with lime.
7. $\frac{3}{32}$ gr. alizarine and $\frac{1}{32}$ gr. rubiacine in combination with lime.
8. $\frac{1}{8}$ gr. alizarine and $\frac{1}{2}$ gr. pectic acid.
9. $\frac{1}{8}$ gr. alizarine and $\frac{1}{2}$ gr. pectic acid in combination with lime.
10. $\frac{1}{8}$ gr. alizarine and $\frac{1}{8}$ gr. alpha-resin.
11. $\frac{1}{8}$ gr. alizarine and $\frac{1}{8}$ gr. alpha-resin in combination with lime.
12. $\frac{1}{8}$ gr. alizarine and $\frac{1}{8}$ gr. beta-resin.
13. $\frac{1}{8}$ gr. alizarine and $\frac{1}{8}$ gr. beta-resin in combination with lime.
14. $\frac{1}{8}$ gr. alizarine and $\frac{1}{8}$ gr. rubian.
15. $\frac{1}{8}$ gr. alizarine and $\frac{1}{8}$ gr. rubian in combination with lime.

Now the following results were obtained :—No. 1 was everything that could be desired in regard to all the colours. No. 2 was of course only half as dark. No. 3 was lighter than No. 1, and the white parts had a pink hue. No. 4 was a little darker than No. 3, but not as dark as No. 1. No. 5 was much inferior to No. 1; the red had an orange hue, the purple a reddish cast, and the black was

brown, while the white was yellowish. No. 6 was equal to No. 1, but not darker, and in no respect superior. No. 7 was about equal to No. 4. No. 8 had almost no colour at all; the red, the purple and the black were mere tinges of colour, such as might probably have been produced by the tenth part of the quantity of alizarine employed, if no pectic acid had been present. No. 9 was again equal to No. 1. No. 10 was lighter than No. 1, the purple especially being pale and reddish, while the white parts were yellowish. No. 11 was equal to No. 1, but not superior. No. 12 was exactly the same as No. 10, the purple having a disagreeable reddish cast, while the white parts were yellowish. No. 13 was again equal to No. 1. No. 14 and 15 did not differ from one another, and were equal to No. 1. Hence we may draw the following conclusions:—Alizarine produces the greatest effect in dyeing when used alone. The addition of lime, even in very small quantities, does not increase its tinctorial power, but on the contrary neutralizes the effect of that portion with which it combines. Rubiacine, the alpha-resin and the beta-resin, in a free state, when used in conjunction with alizarine, are injurious in about the same degree: they weaken the red, the black, and especially the purple, while they render the white part yellowish. In combination with lime these substances do not increase the tinctorial power of alizarine, they merely allow it to act without hindrance. Pectic acid almost destroys the effect of alizarine. Pectate of lime is perfectly indifferent. Rubian in a free state, and in combination with lime, has neither a beneficial nor an injurious effect. Of all the substances therefore contained in madder, none is of use in dyeing but alizarine, while all the others are injurious when in a free state. That which is the most hurtful is pectic acid. When alizarine and pectic acid are present together in the dye-bath, the pectic acid having most affinity for bases, combines with the alumina and peroxide of iron, and the alizarine crystallizes out when the bath cools, as I noticed in performing the experiment No. 8. The same is without doubt the case when using rubiacine or the resins. The alumina and peroxide of iron combine with these substances to the exclusion of the alizarine; and these compounds are either colourless, or have a poor and unsightly colour. The use of lime is therefore easily explained; it serves, not to increase the tinctorial power of the colouring matter, but to combine with and render harmless the substances which are injurious in a free state. Now if we treat madder with muriatic or sulphuric acid, we remove all the lime and magnesia from it; the pectic acid, the rubiacine and the resins become free; and if we wash with water, the muriatic or sulphuric acid is certainly removed; but those substances being but little soluble in cold water, remain and destroy the effect of the alizarine in dyeing. But if previous to dyeing we add lime, the pectic acid, the rubiacine and the resins being more electro-negative than alizarine, combine with the strongest base, which is the lime; and the alizarine, which is less electro-negative, combines with the weakest bases, viz. the alumina and peroxide of iron. If we

add an excess of lime, then of course the alizarine also combines with the lime, and the alumina and peroxide of iron having no free body to combine with, remain colourless. The process is thus brought into harmony with our previous knowledge of the relative affinity of acids and bases. It is probable that lime is not absolutely necessary for the success of the operation, and that it might be replaced by potash, soda, magnesia or baryta; but as lime is the cheapest substance that can be used for the purpose, it would be of no practical importance to find a substitute for it.

I have in the preceding remarks left xanthine out of consideration. During the process of madder-dyeing this substance no doubt becomes oxidized, and deposits the brown substance mentioned above, on all parts of the cloth. This substance, together with the pectic acid, the rubiacine and the resins, are removed afterwards by passing the cloth through a boiling solution of soap. The alkali of the soap dissolves these substances, which have more affinity for alkalies than alizarine, while the fat acid remains on the cloth in combination with the alizarine, the alumina and the peroxide of iron.

In order to prove analytically that alizarine is the substance which produces madder colours, I took several yards of cloth which had been dyed purple with madder, but not soaped, and treated it with muriatic acid, which removed the oxide of iron, and left an orange-coloured substance on the cloth. After washing the cloth in cold water until all the acid had been removed, it was treated with caustic alkali. The brownish-red solution thus obtained was supersaturated with an acid, and the reddish-brown precipitate formed was thrown on a filter and well-washed with cold water: it was then treated with boiling alcohol. The alcoholic liquid, which was dark yellow, was spontaneously evaporated, and gave crystals of alizarine mixed with a powder resembling beta-resin, and a few yellow micaceous plates, which were probably rubiacine. There remained a brown residue insoluble in alcohol, part of which dissolved in boiling water, and proved to be pectic acid. On treating some cloth which had been dyed with madder, and then soaped, with muriatic acid as before, and then with caustic alkali, I obtained a purple solution, in which acids produced a yellow precipitate. This precipitate was treated with boiling alcohol like the other; it gave a yellow liquid, which on evaporation afforded crystals of alizarine, together with white masses of fat acid. Hardly any residue remained undissolved by the alcohol.

The preceding observations have a great bearing on the manufacture and treatment of garancine. Garancine is the technical name for a preparation of madder, which is obtained by treating madder with hot sulphuric acid until it has acquired a dark brown colour, then adding water, straining and washing until all the acid is removed. The advantages which garancine has over madder are, that it dyes finer colours, that the part destined to remain white does not acquire any brown or yellow tinge, and that its tinctorial power is greater than that of the madder from which it has been prepared. These effects have been attributed to various causes. It has been asserted

that the sulphuric acid destroys the gum, the mucilage, the sugar, &c., and leaves the colouring matter unaffected; hence the greater beauty of garancine colours. To account for the greater proportional effect of garancine, it has been said that a part of the colouring matter is enclosed in cells of the wood, so that it cannot be dissolved by water, and that the sulphuric acid destroys the wood and liberates the colouring matter. To these views it may be objected, that concentrated sulphuric acid, though it does not affect alizarine, does not destroy any of the injurious substances in the root except the xanthine, while the rubiacine, the resins, and the pectic acid, escape its action: and as far as the wood is concerned, I can affirm that the operation succeeds equally well if acid be taken of such dilution as not to destroy woody fibre. I think that the superiority of garancine can only be attributed to two causes. In the first place, since, as I have shown above, there is a quantity of colouring matter in the root combined with lime and magnesia, by which it is rendered insoluble and incapable of dyeing, one effect of the acid is to remove this lime and magnesia, and to set the alizarine at liberty, which is then capable of application. In the second place, the xanthine, which has an injurious effect in madder-dyeing, is removed by washing with cold water, since it is not precipitated by acids, while the whole of the alizarine remains. If hot acid is employed, then the xanthine, or a part of it, is converted into that dark green substance which I have mentioned above as the product of the action of muriatic and sulphuric acid on xanthine; hence the dark colour of garancine, which is not owing to the charring of the woody fibre, as sometimes asserted. It must be remembered however that the rubiacine, the resins and the pectic acid, as well as the alizarine, remain uncombined after treatment with acid. Hence it becomes necessary to add some base with which these substances may combine, so as not to interfere with the action of the alizarine. I believe it is the practice of garancine manufacturers to employ soda for this purpose.

I think it would be better to use a small quantity of lime-water.

I may state in conclusion that the experiments described in this and the last report were made with Avignon madder. The constituents and properties of Dutch madder, which is of rather a different nature, remain to be examined.

I have been lately engaged in examining the colouring matter of fustic, which I have prepared in a state of purity, but the investigation is not sufficiently advanced to justify me in making known the results on the present occasion.

XXIX. Notices respecting New Books.

Introduction to Meteorology. By DAVID PURDIE THOMPSON, M.D.
Blackwood and Sons.

THERE is no branch of physical science which has made so little progress, and of whose laws our knowledge is so limited, as that of Meteorology. Hence every encouragement should be given

Phil. Mag. S. 3. Vol. 35. No. 235. Sept. 1849. Q

to every undertaking which endeavours to increase our knowledge of meteorological science, either by improving instruments themselves, their positions in observations, accuracy in observing, or the devotion of the great labour necessary for their reduction to useful results, or by elementary publications aiming at precision on all these points.

Meteorology has been greatly slighted. By the term Meteorology we do not mean the mere raw observations of phænomena, but the systematic observations of different subjects of research, their complete reduction and combination with simultaneous observations, and, as far as possible, the deduction of mathematical formulæ which will represent the results of the observations. It must be confessed that there have been many meteorological journals kept by ardent observers, and that some of them have extended over long periods of time; yet there has been a total want of combination of observations taken simultaneously at different places. The few results, therefore, which have been published from the various journals, bear the stamp of local influence. Each observer has confined his attention to his own observations; which, if conducted in the best possible way and with the best possible instruments, must exhibit the peculiarities of the place of observation, but to an unknown amount.

Within the last few years an attempt has been made to remove these evils. The establishment of various observatories in the colonies, that at the Royal Observatory, Greenwich, that at Makerstoun, near Edinburgh, by Sir Thomas Brisbane, and more recently the organization of many observers in England by Mr. Glaisher, the results being published quarterly by the Registrar-general, are important steps in the right direction.

Such is the present state of meteorology, that facts are to be assembled as observed, and principles established as soon as possible. We could wish to see the present system greatly extended; the important results which we then might fairly expect would be of the utmost value in various ways.

The following abstract of the work by Dr. Thompson may perhaps assist our readers to judge of the additions made by it to our present knowledge of meteorological literature and science.

It opens with an introductory sketch of meteorology, and treats of the ancient superstitions respecting meteorological phænomena, and the supposed power of the ancients in controlling such.

In the *first* chapter are explained the opinions of the ancients upon the atmosphere, and the advance of our knowledge regarding it to the present time. This leads to a discussion upon chemistry, and the connexion of chemical and atmospherical phænomena; the figure of the atmosphere, its density; the different temperatures at which water boils at different elevations; effects of elevation upon respiration; variations in the reading of the barometer, with many anecdotes, and a curve showing the mean monthly reading of the barometer at Greenwich, deduced from the observations taken there during the years 1841–1845, fill the *second* chapter.

The *third* chapter opens with an interesting extract from Ossian, and treats upon the sun and the solar beams, upon temperature in

general, particularizing the hours whose temperature agrees with that of the mean for different places.

The *fourth* traces the direction of isothermal lines; speaks of the mean annual temperature at various places; and gives a curve of the mean monthly temperature at Greenwich, deduced from the observations taken at the Royal Observatory in the years 1841, 1842, 1843 and 1844, but which curve differs from the numbers at the foot of the diagram. The numbers are correct. In this chapter the effects of heat and cold are treated, and many interesting anecdotes are mentioned.

The *fifth* speaks of the colour of the atmosphere, of refraction, twilight, polarization of light, &c.

The *sixth* treats of evaporation and vaporization.

The *seventh* treats of dew and radiation of heat, particularizing the experiments of Dr. Wells, but mentions no subsequent English experiments; of mists, fogs and clouds.

The *eighth* is employed upon rain, in considerable detail. The *ninth* is devoted to the consideration of hail, snow, glaciers, &c. The *tenth* to the rainbow, and to all the phenomena connected with the cirrostratus cloud. The mirage is the principal subject of the *eleventh*, and lightning that of the *twelfth*. The *thirteenth* chapter relates to meteors in general. The *fourteenth* to the aurora borealis and to magnetism. The *fifteenth* treats of the direction and strength of the wind. The *sixteenth* is occupied by particulars respecting the simoom and the sirocco; the *seventeenth* by whirlwinds, great storms and hurricanes; and the *eighteenth* by popular prognostications of the weather.

In each chapter a great number of instances of the class of phenomena is collected. Such are the parts of this work; and we cannot doubt that the public will receive it with favour. It is amusing; there are many interesting anecdotes interspersed; it is written in a pleasing style; and great taste has been exhibited in the aptness and beauty of some of the poetic quotations.

The chapter on instruments, which appears as an appendix, is the least interesting and satisfactory portion of Dr. Thompson's work.

The account given of the barometer is merely popular; and the sketches given of this instrument do not include that of a standard. A large space is devoted to the aneroid barometer; which, beautiful as it is for domestic and ordinary purposes, is totally unfitted for meteorological or experimental inquiry. Dr. Thompson gives the impression that the aneroid barometer is as accurate in its indications as a perfect mercurial barometer; in fact, says that the mean difference of the readings of the two, from 109 simultaneous comparisons, is only two-thousandths of an inch, this result being deduced from the observations published by Mr. Dent on the aneroid barometer. But Mr. Dent in his pamphlet only asserts that the comparisons were made with two of the most expensive and perfect mercurial barometers; neither of which is described or again mentioned. The readings by these barometers do not seem to have been corrected for temperature; so that the near agreement of the readings only indi-

cates that the correction for temperature of this particular aneroid was nearly of the same amount as that of the mercurial barometer, even supposing it to be free of index-error. In the second table of Mr. Dent, the readings of a maximum and minimum thermometer are given, neither of which were probably true at the time the observation was taken.

The sympiesometer is fully described, an instrument that is not used in meteorological investigations; whilst the dry and wet bulb thermometers are not described at all, which are in use in every observatory.

The rain-gauge figured in the book is one with a float and staff, of the worst possible form; and if made as sketched in the book, would give erroneous results, particularly when the wide staff was high, and thereby increasing to a considerable extent the receiving surface of the gauge.

No mention whatever is made about the necessity of having good instruments, and such that have been compared with standards before use. No instructions are given as to the making of observations, or of the method of recording them; and yet in any work bearing the title of an Introduction to Meteorology, these ought surely to be given, and particularly when one chapter is devoted to instruments. The work should more properly be called, a Treatise upon Meteorological Phenomena; and even in this particular it is incomplete. Although a multitude of facts are collected and detailed, and some of a recent date, no mention whatever has been made of the fine series of results derived from the Makerstoun observations by Sir Thomas Brisbane, and published in the Transactions of the Royal Society of Edinburgh; neither of any of the meteorological papers of results published in the Philosophical Transactions for several years past; and the references to the Greenwich volumes are very few, and none since the volume for 1844.

We shall conclude with one remark only. Dr. Thompson in his concluding paragraph says, that the instruments of which he has spoken are the chief instruments used by meteorologists. In our most active meteorological observatories not half the instruments here described are used at all; and the instruments actually required are few in number: these should be good; and before use, should pass through the hands of a gentleman accustomed to them. In fact the general laws which govern atmospheric phenomena can be found only by accurate deductions from the observations of such instruments.

XXX. Proceedings of Learned Societies.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxxiv. p. 458.]

April 23, **O**N the Variation of Gravity at the Surface of the Earth. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

In the theory of the figure of the earth on the hypothesis of ori-

ginal fluidity, a simple expression is obtained for the variation of gravity along the surface, which contains the numerical relation between the ellipticity and the ratio of polar to equatoreal gravity, known as Clairaut's theorem. The demonstration, however, of this expression does not require the hypothesis of original fluidity, if the spheroidal form of the surface and its perpendicularity to the direction of gravity be assumed as results of observation. On the hypothesis merely that the earth consists of nearly spherical strata of equal density, Laplace has established a connexion between the form of the surface, regarded as a surface of equilibrium, and the variation of gravity along it; and in the particular case in which the surface is an oblate spheroid of small ellipticity, having its axis of figure coincident with the axis of rotation, the expression which results for the variation of gravity is identical with that which is obtained on the hypothesis of original fluidity. The object of the author in the first part of this paper is to obtain the general connexion between the form of the surface and the variation of gravity along it, by an application of the doctrine of potentials, without making any hypothesis whatsoever respecting the distribution of matter in the interior of the earth.

The latter part of the paper was devoted to the consideration of the irregularities produced in the variation of gravity by the irregular distribution of land and sea at the surface of the earth. The author has shown why gravity should appear less on continents than on small islands situated at a distance from any continent, which is a circumstance that has long since been observed. The result is accounted for by the elevation of the sea-level produced by the attraction of a continent, in consequence of which a station on a continent is further removed from the centre of the earth than it appears to be. It is shown also that the numerical value of the earth's ellipticity, which has been deduced from pendulum experiments, is somewhat too great, in consequence of the undue proportion of oceanic stations in low latitudes, among the group of stations at which the observations were made which have been employed in the discussion.

The author has given formulæ whereby observed gravity may be corrected for the irregularities of the earth's surface. These formulæ require a knowledge, or at least an approximate knowledge, of the height of the land and the depth of the sea throughout the earth's surface. The sign and magnitude of the difference between observed gravity, and gravity calculated on the hypothesis of the earth's original fluidity, appears on the whole to depend on the insular or continental character of the station at which the observation has been taken. This circumstance renders it probable, that if observed gravity were corrected for the irregular attraction due to the irregular distribution of sea and land throughout the whole surface of the earth, the result would agree far better with gravity calculated on the hypothesis of original fluidity.

May 7.—Additional Note to a Memoir on the Intrinsic Equation of Curves. By Dr. Whewell.

This note contained an extension of a theorem discovered by John

Bernouilli, and demonstrated by Euler, to this effect: that if from any *rectangular* curve a string be unwrapped, and from the curve so described again a string unwrapped, and so on perpetually and alternately in opposite directions, the curves constantly tend to the form of the common cycloid. The extension is to this effect: that if the original curve be not rectangular, the curves perpetually tend to the form of an epicycloid or hypocycloid, according as the angle is greater or less than a right angle.

May 21.—Discussion of a Differential Equation relating to the breaking of Railway Bridges. By G. G. Stokes, M.A., Fellow of Pembroke College.

In August 1847 a Royal Commission was appointed “for the purpose of inquiring into the conditions to be observed by engineers in the application of iron in structures exposed to violent concussions and vibration.” Among other branches of inquiry, the members of the Commission have lately been making experiments on the motion of a carriage, variously loaded in different experiments, which passed with different velocities over a slight iron bridge; the object of the experiments being to examine the effect of the velocity of a train in increasing or decreasing the tendency of a bridge over which the train is passing to break under its weight. The remarkable result was obtained, that the deflection is in some cases much greater than the central statical deflection, and that the greatest deflection takes place after the body has passed the centre of the bridge. In investigating the theory of the motion, reducing the problem to the utmost degree of simplicity by regarding the moving carriage as a heavy particle, and neglecting the inertia of the bridge, Professor Willis, who is a member of the Commission, was led to a differential equation of the form

$$\frac{d^2y}{dx^2} = a - \frac{by}{(2cx - x^2)^2},$$

where x, y are the horizontal and vertical co-ordinates of the moving body, $2c$ is the length of the bridge, and a, b are certain constants. Professor Willis requested the author's consideration of this equation, with a view to obtain numerical results, and to determine, if possible, the velocity which produces a maximum deflection.

The author has expressed y in a series according to ascending powers of x , which is convergent when $x < 2c$. The convergency, however, becomes very slow when x approaches the limit $2c$; and the series does not point out the law according to which $f(x)$ or y approaches its extreme value 0 as x approaches $2c$. When the constant term in the second member of the preceding equation is omitted, the equation may be integrated in finite terms; and consequently the variables can be separated in the actual equation, so that $f(x)$ can be expressed explicitly by means of definite integrals. In this way the author has obtained $f(2c-x) - f(x)$ in finite terms, so that the numerical value of $f(x)$ may readily be obtained from $x=c$ to $x=2c$, after it has been calculated from the series from $x=0$ to $x=c$: and between these limits the series is very convergent, being ulti-

mately a geometric series with a ratio $\frac{x}{2c}$. The author has also investigated a series proceeding according to ascending powers of $c-x$, which converges more rapidly than the former when x approaches c . By the use of these two series, $f(x)$ may be calculated by means of series which are ultimately geometric series, with ratios ranging from 0 to $\frac{1}{4}$.

The unsymmetrical form of the trajectory, and the largeness of the deflection produced by the moving body, come out from the investigation. By means of the numerical values of $f(x)$ the author has drawn a figure representing the trajectory for four different velocities. The expression for the central deflection, however, becomes infinite when x becomes equal to $2c$, which shows that it is necessary to take into account the inertia of the bridge; although, if the bridge be really light, the solution obtained when the inertia of the bridge is neglected may be sufficiently exact for the greater part of the body's course.

ROYAL SOCIETY.

[Continued from p. 154.]

June 14, 1849.—The President announced, that in accordance with the resolution of the Society, requesting him to communicate the thanks of the Society to the Government of the United States for the steps taken by them to ascertain the fate of the Expedition under Sir John Franklin, he had addressed the following letter to His Excellency the American minister:—

MY DEAR SIR,

3 Connaught Place, June 8, 1849.

I have the honour to inform you that at the annual meeting of the Royal Society, held the 7th inst., a communication was read from Admiral Sir Francis Beaufort, in which he apprised the Society that the American Government had nobly undertaken to send an expedition in search of Sir John Franklin. Upon which a vote of thanks was moved by Sir Charles Lemon, seconded by Lord Northampton, and carried with the utmost enthusiasm, expressive of the gratitude of the Royal Society to the American Government, and of their deep sense of the kind and brotherly feeling which had prompted so liberal an act of humanity. Allow me to assure you, that it is peculiarly gratifying to me to have the honour of being the humble instrument in conveying to you the thanks of the Royal Society on this occasion, and permit me to express a hope that this most generous act of the United States may, if possible, draw closer the bonds of friendship between the two kindred nations.

That the United States may continue to progress with the same extraordinary rapidity in the arts of peace and civilization, and to hold

the same high place in the science and literature of the world, is I am sure the anxious desire of the Royal Society.

I have the honour to be,

My dear Sir,

Your most obedient humble Servant,

ROSSE, P.R.S.

“On Carbonate of Lime as an ingredient of Sea-water.” By John Davy, M.D., F.R.S. Lond. & Ed., Inspector-General of Army Hospitals, &c.

The manner in which limestone cliffs rising above deep water are worn by the action of the sea, as it were by a weak acid, such as we know it contains, viz. the carbonic—the manner, further, in which the sand on low shores where the waves break, becomes consolidated, converted into sandstone, by the deposition of carbonate of lime from sea-water owing to the escape of carbonic acid gas,—are facts clearly proving that carbonate of lime is as a constituent of sea-water neither rare of occurrence, nor unimportant in the œconomy of nature, inasmuch as the phænomena alluded to,—the one destructive, the other restorative,—have been observed in most parts of our globe where geological inquiry has been instituted.

Reflecting on the subject, it seemed to me desirable to ascertain whether carbonate of lime as an ingredient of sea-water is chiefly confined to the proximity of coasts, or not so limited enters into the composition of the ocean in its widest expanse.

On a voyage from Barbados in the West Indies to England in November last (1848), I availed myself of the opportunity to make some trials to endeavour to determine this, the results of which I shall now briefly relate.

First, I may mention that water from Carlisle Bay in Barbados, tested for carbonate of lime, gave strong indications of its presence; thus a well-marked precipitate was produced by ammonia, after the addition of muriate of ammonia in excess, that is, more than was sufficient to prevent the separation of the magnesia which enters so largely into the composition of sea-water; and a like effect was produced either by boiling the water so as to expel the carbonic acid, or by evaporation to dryness and resolution of the soluble salts.

On the voyage across the Atlantic, the test by means of ammonia and muriate of ammonia was employed, acting on about a pint of water taken from the surface. The first trial was made on the 15th of November, when in latitude $20^{\circ} 30' N.$, and longitude $63^{\circ} 20' W.$, more than a hundred miles from any land; the result was negative. Further trials were made on the 22nd of the same month in lat. $32^{\circ} 53'$, long. $45^{\circ} 10'$; on the 24th, in lat. $36^{\circ} 23'$, long. $37^{\circ} 21'$; on the 25th, in lat. $37^{\circ} 21'$, long. $33^{\circ} 34'$; on the 26th, in lat. $38^{\circ} 28'$, long. $30^{\circ} 2'$; on the 27th, when off Funchal of the Western Islands, in lat. $38^{\circ} 32'$, long. $28^{\circ} 40'$, about a mile and a half from the shore, the water deep blue, as it always is out of soundings; in all these instances likewise the results were negative; the transparency of the water was nowise impaired by the test applied. The last trial was made on

the 3rd of December, when in the Channel off Portland Head, about fifteen miles; now, slight traces of carbonate of lime were obtained, a just perceptible turbidness being produced.

The sea-water from Carlisle Bay, the shore of which and the adjoining coast are calcareous, yielded about 1 per 10,000 of carbonate of lime, after evaporation of the water to dryness, and the resolution of the saline matter. A specimen of water taken up on the voyage off the volcanic island of Fayal, about a mile from land, yielded a residue which consisted chiefly of sulphate of lime, with a very little carbonate of lime,—a mere trace; acted on by an acid it gave off only a very few minute air-bubbles. A specimen taken up off Portland Head, about fifteen miles, yielded on evaporation and resolution of the saline matter only a very minute residue, about $\frac{1}{4}$ only per 10,000; it consisted in part of carbonate and in part of sulphate of lime.

What may be inferred from these results? Do they not tend to prove that carbonate of lime, except in very minute proportion, does not belong to water of the ocean at any great distance from land? And, further, do they not favour the inference, that when in notable proportion, it is in consequence of proximity to land, and of land, the shores of which are formed chiefly of calcareous rock? In using the word proximity, I would not limit the distance implied to a few miles, but rather to fifty or a hundred, as I am acquainted with shores consisting of volcanic islands in the Caribbean sea destitute of calcareous rock, on which, in certain situations, sandstone is now forming by the deposition from sea-water of carbonate of lime.

Should these inferences be confirmed by more extensive inquiry, they will harmonize well with the facts first referred to, the solvent power, on one hand, of sea-water impregnated with carbonic acid on cliffs of calcareous rock in situations not favourable to the disengagement of carbonic acid gas; and the deposition, on the other hand, of carbonate of lime to perform the part of a cement on sand, converting it into sandstone, in warm shallows, where the waves break under circumstances, such as these are, favourable to the disengagement of this gas; and, I hardly need add, that the same inferences will accord well with what may be supposed to be the requirements of organization, in the instances of all those living things inhabiting the sea, into the hard parts of which carbonate of lime enters as an element.

Apart from the œconomy of nature, the subject under consideration is not without interest in another relation,—I allude to steam navigation. The boilers of sea-going steam-vessels are liable to suffer from an incrustation of solid matter firmly adhering and with difficulty detached, liable to be formed on their inside, owing to a deposition which takes place from the salt water used for the production of steam. On one occasion that I examined a portion of such an incrustation taken from the boiler of the "Conway," a vessel belonging to the West Indian Steam Packet Company, I found it to consist principally of sulphate of lime, and to contain a small

proportion only of carbonate of lime. This vessel had been employed previously in transatlantic voyages, and also in intercolonial ones, plying between Bermudas and the Island of St. Thomas, and in the Caribbean sea and the Gulf of Mexico.

The composition of this incrustation, like the preceding results, would seem to denote, if any satisfactory inference may be drawn from it, that carbonate of lime is in small proportion in deep water distant from land, and that sulphate of lime is commonly more abundant. The results of a few trials I have made, whilst rather confirmatory of this conclusion, showed marked differences as to the proportion of sulphate of lime in sea-water in different situations. That from Carlisle Bay was found to contain 11·3 per 10,000. A specimen taken up in lat. $29^{\circ} 19'$ and long. $50^{\circ} 45'$, yielded about 2 per 10,000, with a trace of carbonate of lime. A specimen taken up off Fayal yielded about 9 per 10,000, also with a trace of carbonate of lime. One taken up off Portland Head, about fifteen miles distant, yielded, as already remarked, only ·4 per 10,000, part of which was sulphate, part carbonate of lime.

By certain management, I am informed, as by not allowing the sea-water in the boilers to be concentrated beyond a certain degree, the incrustation, in the instances of the transatlantic steamers, is in a great measure prevented. Perhaps it might be prevented altogether, were sea-water never used but with this precaution, and taken up at a good distance from land, and in situations where it is known that the proportion of sulphate of lime is small. If this suggestion be of any worth, further, more extensive and exact inquiry will be requisite to determine the proportion of sulphate of lime in different parts of the ocean, and more especially towards land. By the aid of the transatlantic steam navigation companies means for such an inquiry may easily be obtained; and it can hardly be doubted that the results will amply repay any cost or trouble incurred.

Lesketh How, Ambleside,
March 29, 1849.

“On the Universal Law of Attraction, including that of Gravitation, as a particular case of approximation deducible from the principle that equal and similar particles of matter move similarly relatively to each other.” By John Kinnersley Smythies, Esq. Communicated by T. F. Ellis, Esq., F.R.S.

After stating the general object of his investigations and explaining the notation he employs, the author enters upon some preliminary geometrical inquiries. He gives the equation between the six right lines drawn between four points in a plane; the solidity of a tetrahedron in terms of its edges: the equation between the cosines of the six angles made by four right lines meeting in a point; and the equation between ten right lines drawn between five points, with some formulæ of verification. Giving some general rules for the transformation and consolidation of series, he transforms the last equation into one involving the solidities of tetrahedrons, and shows how the sign of each tetrahedron in that equation is determined by

its position relatively to the least solid including them all; and then gives the equation between all the right lines drawn between n points.

Having shown that the result of differentiating the product of n variables, m times successively may be derived from the m th power of the sum of the n variables, developed by the polynomial theorem by substituting for every power of each variable its differential of an order numerically the same as the power; and applied the theorem to find the differential of the m th order of the equation between ten right lines drawn between five points; the author gives the first four successive differentials of the same equation in another form.

Proceeding with his investigation he deduces the necessary equation between the distances and central forces of five moving points, and derives from it the general system of equations which determine the motion of any number of spheres in terms of ϕ (the function of the distance according to which the attractive force varies), their masses and mutual distances. After proving that any number of spheres may move so that the central force shall vary directly as the distance, he shows that only certain values of ϕ are possible for an infinite number of spheres, giving the criterion of possibility; and thence that the only possible law of central force for an infinite number of spheres is that in which the force varies directly as the distance.

The author then enters upon some general considerations on the physical impossibility of an universal law, rigorously exact and expressed by equations involving differentials of no higher order than the second, and on the amount of disturbance by extraneous agencies. Having shown how all equations expressed by rectangular coordinates may be transformed into others involving only the mutual distances of the spheres at m equal intervals of time, he gives an equation of differences defining the motion of n points, such that the distances and their differentials of every order not exceeding m may have any assigned values.

After deducing a general formula for transforming equations of differences not exceeding the m th order into equations between the distances at m equal intervals of time, the author applies it to the last equation, and shows that the equations so found are possible for any number of moving points and for every value of m ; and that the most general law, by which the motion of n equal spheres can be determined, so that all move according to the same law at all times, may be found by taking a proper value of m . He then shows that these equations give a method of unlimited approximation to any unknown law; and suggests the mode of extending the solution of the problem to solids of any figure and mass. Finally, he gives the m th differential of the distance between any pair of points moving according to this law, in terms of the differentials of lower orders including the distances.

XXXI. *Intelligence and Miscellaneous Articles.*EXPERIMENTS ON THE NITROGENOUS COMPOUNDS OF THE
BENZOIC SERIES. BY G. CHANCEL.

THESE experiments relate principally to the action of the hydrosulphate of ammonia upon nitrobenzamide. This substance was first obtained by Mr. Field by the action of heat upon the nitrobenzoate of ammonia; but this process does not always succeed, and I have found it most advantageous to prepare it from nitrobenzoic æther and ammonia.

The action of the hydrosulphate of ammonia upon nitrobenzamide dissolved in alcohol is frequently very complex, whilst with an aqueous solution it is very simple; in the latter case there is a considerable deposit of sulphur and of crystals, which according to my analyses contain $C^7 H^8 N^2 O + aq.$ The water of crystallization is expelled between 212° and 248° .

The formation of this body is in conformity with the known reactions of the hydrosulphate of ammonia upon nitrogenized substances; we have, in fact, $C^7 H^6 XNO + 3SH^2 = C^7 H^8 N^2 O + 2OH^2 + 3S^*$; but if the formation of this substance comes within the known reactions, the change of functions which has taken place is hitherto without example in organic chemistry; for it no longer belongs to the benzoic series, but represents carbanilamide or anilamic urea, that is to say a double carbonate of aniline and ammonia less 2 equivs. water ($CH^2 O^3, C^6 H^7 N, NH^3$) — $2OH^2 = C^7 H^8 N^2 O$. This opinion is founded upon the following facts:—When carbanilamide is heated with potash-lime, ammonia is disengaged at a slightly elevated temperature, which I determined in the state of the platinochloride. When heated still more strongly, no more ammonia is given off, but only pure aniline. The following equation will explain this metamorphosis:—



But this equation evidently does not represent the final reaction, and we must distinguish two phases, in

The first, $C^7 H^8 NO + OKH = NH^3 + C^7 H^6 KNO^2$.

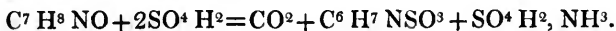
The second, $C^7 H^6 KNO^2 + OKH = C^6 H^7 NCO^3 K^2$.

The salt of potash which is produced in the first phase must be the anthranilate or an isomeric compound, in all cases the true carbanilate.

This experiment proves that carbanilamide contains the consti-

* $X = NO^2 C = 12 H = 1 O = 16 N = 14$, notation of M. Gerhardt.

tvents of 1 equiv. ammonia and of 1 equiv. aniline. The action of sulphuric acid is equally distinct, and supports my opinion; it furnishes, in fact, sulphanilic acid and sulphate of ammonia, with disengagement of carbonic gas:—



By acting with cyanic acid upon aniline, M. Hofmann obtained a substance to which he likewise assigns the composition of anilamic urea. As this chemist has merely announced the production of this substance without describing its properties, I am not able to assert their identity.

Carbanilamide possesses the following properties:—It is soluble in water, alcohol and æther; its alcoholic or æthereal solution quickly acquires a dark red colour, and appears to experience some change, but its aqueous solution is not altered, and furnishes on spontaneous evaporation very beautiful, transparent, flattened prisms of considerable size. The crystals have no odour; they have a cool, very slightly bitter taste, similar to nitre; they contain 1 equiv. water of crystallization, melt at 161° , and are decomposed at a higher temperature, leaving a large amount of carbon.

Carbanilamide, like urea, combines with acids and metallic salts, furnishing crystalline compounds. I have analysed the following:—

The nitrate	$C^7 H^8 N^2 O, NO^3 H$
The argentonitrate	$C^7 H^8 N^2 O, NO^3 Ag$
The chloride.	$C^7 H^8 N^2 O, ClH$

I likewise obtained the hydrargyrochloride and the platinochloride.

In concluding this extract, I may state that I have also analysed the æthers of nitrobenzoic acid; that from alcohol crystallizes in right rhombic prisms of 122° ; my analyses confirm those of M. Kopp; that from pyroligneous æther crystallizes in nearly the same form (118° to 120°); the two æthers are consequently isomorphous.—*Comptes Rendus*, Feb. 26, 1849.

ON THE COMPOSITION OF STEARIC ACID.

BY MM. LAURENT AND GERHARDT.

According to the most recent analyses which have been made in Germany by the process generally in use, and under the direction of M. Liebig, the composition of stearic and margaric acids came to the support of the theory of organic radicals, by assigning to these acids formulæ similar to those of hyposulphuric and sulphuric acids. In a word, these two acids, supposed in the anhydrous state, presented two different stages of oxidation of the same radical,

$R^2 O^3$ and RO^3 , R expressing the composition of a hypothetical radical, margaryle, $C^{34} H^{66} *$.

On taking into consideration the great analogy of the physical characters of these two acids and the perfect identity of their chemical properties, that is to say of their metamorphoses under the influence of reagents, we were led to doubt the accuracy of the formulæ which established between them so great a difference. On the other hand, the formula attributed to stearic acid was in contradiction to the propositions we have advanced respecting the divisibility of the formulæ of organic substances. Either these propositions were false or the composition of stearic acid adopted by the Giessen school was inaccurate. Experiment could alone decide this. Now we have found, in the first place, that stearic and margaric acids have the same atomic weight; on this point we agree with all those chemists who have analysed the salts. We have made seven analyses of stearic acid, derived from four different sources. The differences between the analyses amount for the carbon to one or two-thousandths, and scarcely to one-thousandth for the hydrogen. They lead exactly to the formula of margaric acid, viz. $C^{34} H^{68} O^4$. We have likewise observed, as was announced by M. Chevreul long since, that pure stearic acid may be distilled for the greater part without alteration, and that it behaves in this respect like all the other volatile acids of the same homologous series, as formic, acetic, butyric acids, &c. If, under certain circumstances, this distillation is accompanied by the formation of other products, for instance liquid hydrocarbons, they may be entirely avoided by distilling from 15 to 20 grms. of the pure acid, and interrupting the operation as soon as the last portions acquire a faint brownish tint. Again, if some German chemists have observed in the distilled acid a lower melting-point, which led them to view the product as margaric acid; this decomposition, if it occurs, is effected, according to us, without any decomposition, and can only be the result of some molecular change.

In fact, a greater difference does not exist between stearic and margaric acid than between tartaric and metatartaric acid; they are two physical varieties of one and the same chemical species; margaric acid should be called metastearic acid.

This result is moreover confirmed by all the reactions known of these bodies; the identity of composition and of characters of the substances described by M. Bussy under the names of margarone and stearone; the identity of the results obtained by M. Erdmann in the analyses of the products of anhydrous phosphoric acid upon stearic and margaric acids; lastly, the identity of the atomic weights of these two acids—all these facts now find their natural explanation in this state of isomerism which we have pointed out.

The chemical history of the fatty substances is thus simplified.

* Margaric acid, $C^{34} H^{66} O^3 + H^2 O$; stearic acid, $2(C^{34} H^{66}) O^5 + 2H^2 O$ (bibasic) $= C^{68} H^{136} O^7$, with the oxygen indivisible by 2.

Physiology will be interested in the result, for it does away with that inexplicable and most singular difference which seemed to exist between the composition of the fatty bodies of man and the pig and that of other animal fats.—*Comptes Rendus*, March 26, 1849.

METEOROLOGICAL OBSERVATIONS FOR JULY 1849.

Chiswick.—July 1. Very fine. 2. Light clouds: very fine. 3. Cloudy: slight showers. 4. Cloudy: clear at night. 5. Clear: very fine. 6, 7. Very fine. 8. Very hot: clear at night. 9. Very fine: cloudless. 10. Dusky haze: clear. 11, 12. Very fine. 13. Cloudy. 14. Very fine. 15. Overcast. 16. Very fine. 17. Cloudy: showery. 18. Very fine: cloudy: heavy shower 5 P.M.: showery. 19. Showery. 20. Fine: dense masses of low white clouds: showery. 21. Cloudy: showery. 22. Very fine: overcast. 23. Rain: cloudy. 24. Cloudy: heavy showers in forenoon: excessively heavy rain at night. 25. Fine: cloudy: showers occasionally: heavy showers, with thunder in afternoon. 26. Fine: thunder showers. 27. Overcast: very fine. 28. Very fine: cloudy. 29. Densely overcast. 30. Fine. 31. Very fine: cloudy.

Mean temperature of the month	62°29
Mean temperature of July 1848	62°09
Mean temperature of July for the last twenty-three years ...	63°23
Average amount of rain in July	2·38 inches.

Boston.—July 1—6. Fine. 7. Fine: thermometer 81° from 2 P.M. to 6 P.M. 8—12. Fine. 13—16. Cloudy. 17. Cloudy: rain A.M. 18. Cloudy: rain P.M. 19. Rain: rain A.M. and P.M. 20. Fine: rain A.M. 21. Fine: rain P.M. 22. Fine. 23. Rain: rain A.M. and P.M., with thunder and lightning. 24, 25. Cloudy: rain A.M. 26—28. Fine. 29. Rain: rain A.M. and P.M. 30. Fine. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—July 1. Fine rain: high wind P.M. 2. Rain during night: cleared and fine. 3. Heavy rain and strong wind. 4. Fine: slight shower. 5. Fine: occasional showers. 6. Complete day of rain. 7. Very heavy rain: high flood. 8. Fine and fair: pleasant air. 9, 10. Fine and fair. 11. Very fine summer day. 12. Very warm. 13. Warm, but cooler from east wind. 14, 15. Warm. 16. Warm, but getting cloudy. 17. Heavy rain at night: clear day. 18. Rain. 19. Showers: occasionally fair and warm. 20. Showers: heavy P.M. 21. Fine and fair. 22. Drizzly: showery. 23. Fine A.M.: showery P.M. 24. Showery. 25. Heavy showers: thunder. 26. Warm: slight shower: thunder. 27. Fair: cloudy: cleared P.M. 28. Fair A.M.: rain P.M. 29. Heavy showers. 30. Heavy showers: thunder. 31. Heavy showers, less frequent: thunder.

Mean temperature of the month	57°0
Mean temperature of July 1848	56°5
Mean temperature of July for twenty-five years	58°1
Rain in July for twenty years	3·91 inches.

Sandwich Manse, Orkney.—July 1. Rain: drizzle. 2. Showers. 3. Rain. 4. Bright: fine. 5. Clear: fine. 6. Cloudy: fine: cloudy. 7. Rain: cloudy: showers: bright. 8, 9. Clear: cloudy. 10. Damp: cloudy. 11. Cloudy: fine. 12—14. Fine: fog. 15. Cloudy: clear. 16. Bright: rain. 17, 18. Bright: cloudy. 19. Clear: damp. 20. Drizzle: cloudy. 21. Drizzle: damp. 22. Fine: drops. 23, 24. Cloudy: drizzle. 25. Rain: clear. 26. Bright: damp. 27. Bright: showers. 28. Bright: rain. 29. Clear: showers: fine. 30. Clear: fine. 31. Showers.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; by the Rev. W. Dunbar, at Applegarth Manse, DUMFRIES-SHIRE; and by the Rev. C. Clouston, at Sanduick Manse, ORKNEY.

Days of Month.	Barometer.						Thermometer.				Wind.		Rain.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																					
	Chiswick.		Poston 8 1/2 a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.	Poston. 8 1/2 a.m.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Poston.	Dumfries-shire.	Orkney, Sandwick.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																			
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1.	30.160	29.921	29.63	29.91	29.70	29.61	29.61	79	57	64	51	sw.	sw.	sw.	w.</

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

OCTOBER 1849.

XXXII. *On the Views of the Astronomer Royal respecting the Modification of Sounds by Distance of Propagation. By the Rev. J. CHALLIS, M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*.*

IN the Number of the Philosophical Magazine for last June, the Astronomer Royal has stated it to be his belief that a musical sound may degenerate into a *hiss*, a *buzz*, or a *whisper*, and possibly into a *roar*, by mere distance of transmission. So singular and novel a theory can hardly be expected to pass current without being questioned. I consider myself entitled to call it in question, because a short while before it was promulgated I had proved in this Magazine, by what I conceive to be strict mathematical reasoning, that musical sounds do not by propagation become *unmusical*. The views of the Astronomer Royal profess to be only conjectures, being neither supported by mathematical reasoning, nor borne out by any experiment, and on this account they cannot well be brought under discussion. They are, however, based on a piece of mathematical reasoning which the Astronomer Royal has adopted from Mr. Stokes, and which admits of being discussed. Now I am prepared to show that this piece of reasoning is faulty in its logic, and that it consequently affords no pretext for the conjectures of the Astronomer Royal.

The rule of logic against which Mr. Stokes has offended may be stated as follows:—When the equations applicable to a proposed problem have been formed according to the principles of the department of science to which the problem belongs, and a solution of the equations has been obtained, it is not allowable to accept the solution in part and reject it in part: it is either wholly applicable or not at all. In the

* Communicated by the Author.

instance of the hydrodynamical problem of the propagation of sound in plane-waves, Mr. Stokes has violated the above rule by accepting the analytical solution for certain values of the co-ordinates and the time, and rejecting it for other values. I proceed to make this assertion good.

The following is the enunciation of the problem. Assuming that a medium in which the pressure p and density ρ are related to each other by the equation $p = a^2 \rho$, is capable of propagating waves in such a manner that the velocity and density of the particles of the medium are functions of the distance from a given plane, it is required to determine the motion.

Let v be the velocity of any particle at the distance z from the given plane, and at the time t reckoned from a given epoch. Then by a known process, which it is unnecessary to go through, we have for the case of propagation in a single direction, the general solution,

$$v = a. \text{Nap. log } \rho = f(z - (a + v)t + c),$$

and giving to the arbitrary function a form corresponding to a particular series of waves, we have the definite solution,

$$v = a. \text{Nap. log } \rho = m \sin \frac{2\pi}{\lambda} (z - (a + v)t + c).$$

By these equations the velocity and density are given at all distances from the given plane at any instant, and the solution is in all respects complete. If, to ascertain the rate at which a given velocity and density are propagated, we differentiate the equations supposing v and ρ to be constant, it will be

found that the velocity v and corresponding density ρ are propagated with the velocity $a + v$. The effect of the difference of rate of propagation of different parts of the wave presents itself as a continuous modification of the form of the wave. Mr. Stokes has exhibited this modification by a geometrical curve (see *Phil. Mag.* vol. xxxiii. p. 359), and in so doing has accepted the indications of analysis during an interval equal to $\frac{\lambda}{2\pi m}$, but has rejected all its subsequent indica-

tions. To take a numerical instance, let $a = 916$ feet in a second, $m = 1$ foot in a second, and $\lambda = 10$ feet. Then it would be admitted in this instance, that during an interval of one second and six tenths, in which the wave travels over 1459 feet, the analysis indicates truly, but not for any longer period. This procedure Mr. Airy has signified his approval of by saying, that "Mr. Stokes has shown that a distinct

meaning can be given to the solution up to a certain point in the progress of the wave." If we may believe these two mathematicians, the wave at this point suddenly emancipates itself from the control of analysis, and the subsequent motion can be pursued by no other means than conjecture. Accordingly each has favoured us with his conjectures. Mr. Airy's are contained in the article which has given occasion to these remarks, and Mr. Stokes's in the communication already referred to (Phil. Mag. vol. xxxiii. pp. 352-356).

It is not my intention to enter into any consideration of these conjectures, which, in my opinion, can only be regarded as proofs that the authors of them have fallen into an erroneous course of reasoning. In any legitimate application of analytical calculation to a physical question, a *spontaneous* failure of the analysis, leaving no resource but conjecture, can never occur. Before such necessity has arisen, some fault in logic must have been committed. I do not doubt that I point out the fault committed in the case under discussion by saying, that certain consequences deduced by analysis from the premises are received, while other consequences equally deduced by analysis from the same premises are rejected. The analytical solution of the problem of plane-waves is, as I have already stated, that the velocity v at any point of the wave and the corresponding density are propagated with the velocity $a + v$. In consequence of the different rate of propagation of different parts of the wave, two different states of the medium must occupy eventually the same position at the same time. For instance, a position of maximum velocity is a position of no velocity when the interval t has become $\frac{\lambda}{4m}$; that

is, taking the numerical example before adduced, after the lapse of two seconds and a half, when the wave has travelled through 2290 feet. We are thus brought by the analysis to an absurdity. It is true the absurdity is first consummated

at the end of the interval $\frac{\lambda}{2\pi m}$. Mr. Stokes avails himself of this circumstance to limit his acceptance of the results of analysis to this interval, and avoids the consideration of the absurdity; whereas right reasoning demands, if in any case an absurdity is strictly deduced from premises, that *all* results from the same premises should be rejected. It is in vain to urge, as Mr. Airy does, that the absurdity indicates the transition of the waves from a *musical* to an *unmusical* condition. A *hiss* and a *roar* are matters of experience and possible; they cannot therefore be symbolized by an impossibility. The only legitimate inference from the absurdity is, that the *hypo-*

thesis of the problem is at fault; *that it is not allowable to make the supposition of plane-waves.*

Mr. Airy's views, as I have said, are not borne out by any experiment. On the contrary, an experiment may be adduced which contradicts them. The possibility of hearing distinctly words spoken at a distance, depends on the faithfulness with which the air transmits the impressions made on it by the organs of voice. As the difference between the sound of one letter and that of another corresponds to a difference in the form of the curve representing the succession and magnitudes of the condensations impressed, it is necessary that that form should remain unchanged by distance of transmission in order that words heard at different distances may be the same sounds. The law of transmission expressed by the formula $a + v$, which is the basis of Mr. Airy's speculations, is opposed to this constancy of form. M. Biot, however, has recorded an experiment made at Paris, according to which, words pronounced at one end of a cylindrical tube 3120 feet in length, were perfectly distinct at the other end.

Cambridge Observatory,
August 22, 1849.

XXXIII. *An Investigation on the Chemical Nature of Wax.*
By BENJAMIN COLLINS BRODIE, Esq.*

[Continued from vol. xxxiii. p. 391.]

III.† *On Myricine.*

I HAVE placed the investigation of the Chinese wax between that of the cerotic acid and of the residue of the bees'-wax which remains after that substance has been separated from it. By the saponification of this Chinese wax we procure, as I have shown, an acid identical with the cerotic acid from bees'-wax, and also the alcohol of this acid, so that the chemical history of these substances is closely connected. We have moreover in the Chinese wax to deal with a substance found in nature in a state of great purity, the products of the decomposition of which by alkalies and by heat can readily be prepared and examined. The knowledge of the relation of these products to one another throws great light upon the nature of myricine, which is not a pure substance, and the chemical relations of which are complex.

I have stated that the first extracts of wax with alcohol give with acetate of lead an abundant precipitate in a hot alcoholic

* From the Philosophical Transactions for 1849, part i.; having been received by the Royal Society May 11, and read November 23, 1848.

† Part I. was inserted in vol. xxxiii. p. 217, and Part II. at p. 378 of the same volume of this Journal.—Ed.

solution. This affords us a ready test of the presence of the cerotic acid. The wax may be long boiled with alcohol before the whole of the cerotic acid is removed. If however this process of boiling and decantation be continued, a time will come when the acetate of lead will cease to give any precipitate whatever in the hot alcoholic extract. The residue after this extraction I speak of as myricine. It is advisable to continue for two or three times the operation of boiling and decanting, even after the acetate gives no precipitate, the cerotate of lead not being entirely insoluble in the hot solution.

The myricine thus prepared is a greenish substance of about the consistency of wax, uncrystalline, still possessing a slight smell of wax, and of a melting-point of 64° C. This substance is hardly acted on by dilute potash. It is however saponified by boiling with strong potash, and more readily by an alcoholic solution of the alkali. The saponification may also be effected by melting it with hydrate of potash, as in the case of the Chinese wax. The products are the same in whichever way the operation be conducted.

If the soap from the saponification of the myricine be treated in the same manner as the similar soap from the Chinese wax*, it also will be found to contain two substances, an acid and another substance which is contained in the æther with which the baryta salt is extracted. On attempting to purify these substances respectively by crystallization out of alcohol, æther or absolute alcohol, great variations in the melting-point both of the acid and of the basic substance will be observed. And careful observation shows that these are not, as in the case of the Chinese wax, substances in a state of comparative chemical purity, but are mixtures, both in the case of the acid and of the other matter, of at least two bodies difficultly separable from one another. It is the separation of these substances which gives a peculiar difficulty to the investigation of the nature of myricine.

Although the acid and basic products of the saponification may thus, as in the case of the Chinese wax, be separated by precipitation of the soap by a baryta salt, in the case of the bees'-wax these substances admit of a simpler method of separation, without which method, so difficult is it to wash perfectly out the baryta salt, that I question whether the substances could be obtained pure. The soap, in whatever way the saponification may have been effected, and after the alcohol, if any, used for the saponification has been distilled off, is to be dissolved in a large quantity of water, and the boiling solution decomposed by an acid. The melted mass which re-

* Phil. Mag. vol. xxxiii. p. 381.

sults from this operation, after having been repeatedly boiled out with water, is to be dissolved in a large quantity of hot alcohol. An abundant precipitate appears in the cold fluid from which the solution is to be filtered, and the precipitate repeatedly redissolved and recrystallized out of alcohol. The precipitate will at length be found to consist, almost entirely, of the basic portion of this waxy matter. The alcoholic solution contains the acid.

I shall proceed to give the simplest method by which the pure substances may be obtained, and those experiments which I have made upon their constitution, which I think can leave no doubt upon the mind of the chemist as to the true nature of that matter of which by far the greater portion of the myricine and, indeed, of the wax itself consists.

The first separation of the products of saponification may be made, as I have stated, by combining the acid with baryta and washing out the resulting salts with æther; the basic portion of the products may be obtained as pure by this as by the other method.

Melissine.

If the substance contained in the ætherial solution, with which the baryta salt is washed out, be crystallized out of æther or alcohol, the melting-point will be considerably raised, from below 70° C. to above 80° , by repeated crystallization. The difficulty with which the melting-point was raised, made it evident that the substances contained in the solution were to be separated only by long crystallization and a careful attention to the variations of the melting-points. I made various experiments to discover a satisfactory method of purification. At length I found that if the ætherial solution be filtered while yet warm, and when only a small portion of matter has crystallized out, a substance remains on the filter of a melting-point of 85° C. of a satiny lustre, and of highly crystalline appearance. It is with difficulty that even a small portion of substance can be thus obtained, and it is necessary to use, during the filtration, a hot water apparatus to prevent the precipitation of the whole matter dissolved. I have never been able to succeed in further raising the melting-point of this body, and therefore regard it as pure. In this condition it crystallizes on cooling from the melted state, and its crystallization is marked by striæ parallel to the line of cooling; it being in all respects, but the melting-point, similar in appearance to cerotine as procured from Chinese wax.

I give this method of preparing this substance as it was the first I adopted, and as it can thus be procured in a high state

of purity. I afterwards however discovered the use of rectified coal naphtha as a solvent for these substances, and by far the best and simplest method of procuring the body is by crystallization out of that solvent, of the precipitate from the alcoholic solution which I have before mentioned, as procured by dissolving in alcohol the wax matter obtained by decomposing by an acid the soap from the myricine. By alcohol the basic portion of the saponified myricine is separated from the acids. By naphtha the substance of 85° melting-point is separated from another and probably an analogous body, of which I shall speak hereafter.

This substance gave to analysis the following numbers. The result is the same in whatever way the substance is prepared.

Substance.	CO ₂ .	HO.
I. 0·2685 grm. gave .	0·8075	0·341
II. 0·2597 grm. gave .	0·7839	0·3326
III. 0·278 grm. gave .	0·84375	0·35325
IV. 0·2584 grm. gave .	0·7812	0·325
V. 0·2511* grm. gave .	0·7595	0·3215
VI. 0·2617† grm. gave .	0·7870	0·3295

which give in 100 parts—

	I.	II.	III.	IV.	V.	VI.
Carbon .	82·02	82·40	82·77	82·43	82·48	82·01
Hydrogen .	14·11	14·25	14·11	13·97	14·22	13·99
Oxygen .	3·87	3·35	3·12	3·60	3·30	4·00
	<u>100·00</u>	<u>100·00</u>	<u>100·00</u>	<u>100·00</u>	<u>100·00</u>	<u>100·00</u>

These analyses agree with the formula—

	Atomic weight.	Calculated.
C ₆₀ . . .	360	82·19
H ₆₂ . . .	62	14·15
O ₂ . . .	16	3·66
	<u>438</u>	<u>100·00</u>

This substance I propose to call *Melissine*.

Melissic Acid.

Melissine, heated with lime and potash, as the similar experiment was made with *cerotine*†, is, like that body, con-

* This substance was procured directly from wax, from which it may be obtained and purified in the same manner as from the purified myricine; which is the simplest way of procuring the substance if the other products of saponification are not required.

† This substance was procured from the Ceylon wax mentioned in a former paper.

‡ *Phil. Mag.* vol. xxxiii. p. 381.

verted into an acid. This acid has a similar appearance to the wax acid already described. It has however a much higher melting-point, namely, 88° – 89° C. The preparation of the substance need not be again described.

	CO ₂ .	HO.
I. 0·2655 grm. gave	0·7764	0·3104
II. 0·2507 grm. gave (another preparation)	0·728	0·2507
III. 0·2508 grm. gave	0·7333	0·3077
IV. 0·2396 grm. gave (another preparation)	0·7026	0·2885
V. 0·258 grm. gave		0·3085

which give in 100 parts—

	I.	II.	III.	IV.	V.
Carbon .	79·74	79·19	79·74	79·97	
Hydrogen .	13·00	13·32	13·63	13·40	13·28
Oxygen .	7·26	7·49	6·63	6·63	
	<u>100·00</u>	<u>100·00</u>	<u>100·00</u>	<u>100·00</u>	

These analyses agree with the formula—

	Atomic weight.	Calculated.
C ₆₀ . .	360	79·64
H ₆₀ . .	60	13·27
O ₄ . .	32	7·09
	<u>452</u>	<u>100·00</u>

I prepared the silver salt of this acid in precisely the same manner as was prepared the silver salt of the cerotic acid.

I. 0·6085 grm. gave	0·1175 silver.
II. 0·678 grm. gave	0·1315 silver.
III. 0·58625 grm. gave (another preparation)	0·11575 silver.

which give in 100 parts—

	I.	II.	III.
Silver	19·30	19·39	19·74

	CO ₂ .	HO.
I. 0·4619 grm. of the salt gave	1·0863	0·4464
II. 0·484 grm. of the salt gave	1·13375	0·471

giving in 100 parts—

	I.	II.
Carbon	64·13	63·90
Hydrogen	10·73	10·81
Oxygen and silver .	25·14	25·29
	<u>100·00</u>	<u>100·00</u>

These analyses lead to the formula C₆₀H₅₉O₄ Ag.

	Calculated.
C ₆₀	64.38
H ₅₉	10.55
O ₄	5.77
Ag	19.30
	<hr/> 100.00

The formula therefore of the hydrated acid is C₆₀ H₆₀ O₄. This acid I call *Melissic Acid*.

Chlor-Melal.

By the action of chlorine on melissine a perfectly analogous result is obtained to that obtained by the action of chlorine on cerotene. The substance undergoes also a similar change in appearance, being converted into a resin.

The substance was prepared and analysed with a view to confirming the formula of the body.

	CO ₂ .	HO.
I. 0.4136 grm. gave	0.589	0.175
II. 0.4263 grm. gave	0.602	0.1835

which give in 100 parts—

	I.	II.
Carbon	38.83	38.51
Hydrogen	4.70	4.78
Oxygen and chlorine	56.47	56.71
	<hr/> 100.00	<hr/> 100.00

I. 0.6663 grm. gave 1.4821 grm. of chloride of silver equivalent to 0.3665 grm. of chlorine.

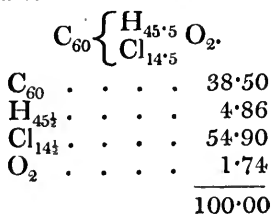
II. 0.6075 grm. gave 1.341 grm. of chloride of silver equivalent to 0.3316 grm. of chlorine.

III. 0.6475 grm. gave 1.4375 grm. of chloride of silver equivalent to 0.3555 grm. of chlorine.

These determinations correspond in 100 parts to—

	I.	II.	III.
Chlorine	55.01	54.58	54.91

These analyses lead to the formula



As in the case of the cerotene, by the action of chlorine two

equivalents of hydrogen are removed without replacement by chlorine, while the further action is an action of substitution, the substance being the analogue of chloral.

The products of the distillation of melissine are analogous to those of the distillation of cerotine. The substance partly distils over unaltered, and is partly, with the loss of water, converted into solid hydrocarbon. Sulphuric acid also combines with it under the same conditions as with the other wax-alcohol.

Palmitic Acid from the Saponification of Myricine.

Melissine is soluble with such great difficulty, in every solvent suitable for washing out the baryta salt from the wax soap, that its separation from the acid cannot in this manner be effected. It may however be separated by simple crystallization. The alcoholic solution (p. 246) from which the melissine has crystallized out, after having been considerably concentrated and again filtered from any precipitate produced on cooling, contains hardly a trace, if any, of that substance. The acids are very soluble in alcohol, and it is only on great concentration that they crystallize from that solvent. The alcohol is to be distilled off to the point of crystallization, and the first portions only of the fat acid selected for the preparation of the pure substance. The acid is to be boiled with potash, combined with baryta, and washed out with æther.

On decomposing the baryta salt with hydrochloric acid, a fat acid separates, having the appearance of margaric or palmitic acid, which latter body is in truth the principal acid of the wax. It is however mixed with another acid of a lower melting-point, for which reason it is desirable, as I have mentioned, to use in its preparation only the first crystallization of the acid. From this other body it is separable with the greatest difficulty; but by long-continued crystallization from æther, an acid may be obtained of the melting-point of 62°C ., beyond which point it cannot be raised. This acid gave to analysis the following results:—

		CO_2 .	HO.
I.	0.2486 grm. gave . .	0.6877	0.278
II.	0.2605 grm. gave . .	0.7145	0.290
III.	0.2542 grm. gave . .	0.6937	0.2847
giving per cent.—			
	I.	II.	III.
Carbon . .	75.42	74.80	74.43
Hydrogen . .	12.43	12.36	12.43
Oxygen . .	12.15	12.84	12.14
	<hr/> 100.00	<hr/> 100.00	<hr/> 100.00

The silver salt was made as in the other cases by precipitation from the ammoniacal solution of the acid.

I.	0.6885	gram. of this salt gave	0.2005	silver.
II.	0.66025	gram. of the same gave	0.1920	silver.
III.	0.623	gram. of another preparation gave	0.182	silver.
IV.	0.609	gram. of the same gave	0.17625	silver.
V.	0.671	gram. of another preparation gave	0.197	silver.
VI.	0.744	gram. of the same gave	0.2185	silver.

giving in 100 parts—

I.	II.	III.	IV.	V.	VI.
29.12	29.23	29.21	28.94	29.35	29.36

		CO ₂ .	HO.
I.	0.4458 grm. of the first preparation gave	0.869	0.3495
II.	0.4463 grm. of the same preparation gave	0.870	0.3555
III.	0.5896 grm. of the second preparation gave	0.7545	0.3065

which correspond in 100 parts to—

	I.	II.	III.
Carbon	53.16	53.22	52.82
Hydrogen	8.70	8.85	8.75
Silver and oxygen	38.14	37.93	38.43
	<hr/> 100.00	<hr/> 100.00	<hr/> 100.00

The silver salt is by no means insoluble in the ammoniacal solution, so that in the making of the salt by this method a certain separation of the substance is effected. If any impurity were presented, it probably would be detected on analysing the acid as again separated from the silver salt.

		CO ₂ .	HO.
I.	0.2523 grm. of the acid thus separated gave	0.6970	0.285
II.	0.228 grm. of the same gave	0.6255	0.257

giving in 100 parts—

	I.	II.
Carbon	75.38	74.82
Hydrogen	12.56	12.52
Oxygen	12.06	12.66
	<hr/> 100.00	<hr/> 100.00

These analyses, as well as those of the acid previous to combination with silver, agree with the formula of palmitic acid, $C_{32}H_{32}O_4$, with which substance the melting-point of the acid also identifies it. The calculated numbers in parts per cent. of the acid and silver salt are—

C ₃₂	192	75.0
H ₃₂	32	12.5
O ₄	32	12.5
		<hr/> 256	<hr/> 100.0

C ₃₂	. .	192	52·8
H ₃₁	. .	31	8·5
O ₄	. .	32	9·0
Ag	. .	108·1	29·7
		<hr/> 363·1	<hr/> 100·0

Distillation of Myricine.

The discovery of the cerotic acid rendered it evident that in order to obtain the products of distillation of myricine, and especially the acids in a state of purity, it was necessary first to remove that body and to distil only the residue of the wax. I give the results of this experiment made with myricine. The first portions of the distillate consist almost entirely of acids, the latter of hydrocarbons. During the distillation a smell of butyric acid may be perceived. This however appeared to me to diminish when the boiling of the wax with alcohol had been very long continued. It is possible to effect nearly a complete separation of the acids and the hydrocarbons by distillation. It is however not advisable to proceed in this manner, but it is best after boiling the distillate with water to saponify the whole by potash. The soap may be removed by a syphon from the hydrocarbons which float on the surface.

Palmitic Acid from the Distillation of Myricine.

The acid, having been purified in the usual manner by washing out the baryta salt with æther, and the subsequent methods of purification, presents an appearance similar to the acids obtained by saponification. By crystallization the melting-point may be raised to 62° C.

	CO ₂ .	HO.
I. 0·2592 grm. of this acid gave	0·7165	0·2931
II. 0·250 grm. of this acid gave	0·6865	0·27925
III. 0·2775 grm. of this acid gave	0·75925	0·311

These analyses correspond in 100 parts to—

	I.	II.	III.
Carbon . .	75·39	74·89	74·61
Hydrogen . .	12·58	12·40	12·45
Oxygen . .	12·03	12·71	12·94
	<hr/> 100·00	<hr/> 100·00	<hr/> 100·00

The silver salt of this acid, prepared as before, gave the following results:—

- I. 0·5006 grm. of the substance gave . . 0·1479 silver.
 II. 0·2295 grm. of another preparation gave 0·0685 silver.

which correspond in 100 parts to—

	I.	II.	
Silver . . .	29·54	29·84	
		CO ₂ .	HO.
0·3505 grm. of the same salt gave .		0·6873	0·2758
which gives in 100 parts,—			
Carbon	53·47		
Hydrogen	8·74		
Oxygen and silver	37·79		
	100·00		

These numbers prove the identity of the acid from the distillation of myricine with that obtained from the saponification of that substance (p. 251).

There are great difficulties in the way of obtaining even a sufficient quantity of this acid for the determination of its formula. To obtain even a very small portion of it of which the purity may be relied on, it is necessary to operate on a large quantity of the impure acid: for the preparation of this pure myricine is required, free from cerotic acid, which it is not easy to get in any quantity.

These difficulties have prevented me making any further experiments with this acid, the identity of which however with palmitic acid, as obtained by Fremy and Stenhouse from palm oil, and by Sthamer from Japan wax, is made out. I subjoin, for the sake of comparison, the silver determination of the silver salt of the palmitic acid as obtained by these chemists.

	Frémy*.			Stenhouse†.	Sthamer‡.
Silver, per cent.	29·60	29·23	29·42	29·28	29·51

This acid appears also to be the same as the acid obtained by Varrentrapp§ from the oxidation of oleic acid by means of lime and potash, which also had the melting-point of 62° C. The silver determinations of this acid gave as the per-centage of silver,

29·27 29·45 29·13,

numbers identical with my own.

Melene.

It is well known that one of the principal products of the dry distillation of wax is a solid hydrocarbon. Ettling, who

* Liebig's *Annalen*, vol. xxxvi. p. 45. Silver determinations, V. VI. VII.

† Ibid. p. 52. The mean of five determinations closely agreeing. This acid melted at 60° C.

‡ Ibid. vol. xliii. p. 342. The mean of three determinations.

§ Ibid. vol. xxxv. p. 209.

first analysed this substance*, concluded from its melting-point, analysis, and general appearance, that it was identical with paraffine, a hydrocarbon then recently discovered by Reichenbach in the products of the dry distillation of wood. The wax hydrocarbon has therefore borne the name of paraffine.

This substance was supposed, from the analyses of Ettling and J. Gay-Lussac, to be isomeric with olefiant gas.

Recently, however, this has been contested by Lewy, who analysed paraffine from various sources, and showed it, as he conceived, to contain a larger amount of hydrogen than had been previously supposed. In truth the average of his analyses gave,—

Carbon	85.03
Hydrogen	14.87
	<hr/> 99.90

numbers inconsistent with the old idea. The question however is, whether M. Lewy experimented with a pure chemical substance, for which there is no guarantee.

My own experiments confirm the analyses of Ettling, and the constitution originally assigned to the substance, to which theoretical considerations also lead. But I cannot see any reason to believe the wax hydrocarbon to be identical with the paraffine of Reichenbach. This name of paraffine has been applied indiscriminately to the whole class of solid hydrocarbons, which have, or have nearly the formula $C_m H_m$, the identity of which has been taken for granted, in the absence of any true knowledge as to the chemical nature of the substances from the decomposition of which by heat they are produced. The different melting-points however of these substances point out to us at once a distinction between them. The paraffine of M. Lewy melted at $46^{\circ}8$. A specimen of the paraffine of wood given to me by Professor Liebig, and which that gentleman received from Reichenbach, its discoverer, melted at $43^{\circ}5$ C.; Ettling's paraffine at 57° to 58° C. I confess it is difficult for me to conceive what substance in a state approaching to purity Lewy analysed from the wax having the melting-point he has given, since nothing is easier than to raise the melting-point of the paraffine from the wax to 56° C., although beyond this any change is effected with difficulty.

Cerine alone gives on distillation hardly a trace of this hydrocarbon, while it forms a principal product of the distillation of myricine. The palmitic acid is separated by saponification,

* Liebig's *Annalen*, vol. ii. p. 259.

and the general preparation of the substance is the same as in the similar case of the cerotene from Chinese wax, to which substance it is closely analogous. If the hydrocarbon from the distillation of the pure myricine, the acids having been boiled out with potash, be pressed out in a press between blotting-paper, it will have a melting-point of about 56° C. This can be raised by further crystallization out of æther to 60° C. The analysis of the substance in this condition shows the presence of some body containing oxygen, in addition to the hydrocarbon.

0.2606 grm. of this substance gave CO_2 0.8094 HO. 0.3402
giving in 100 parts,—

Carbon	84.74
Hydrogen	14.51
Oxygen	0.75
		<hr/> 100.00

Another analysis gave similar results. This led me to prepare the substance in rather a different manner. The paraffine having been carefully pressed out in the manner described, was rectified over potassium, which destroys the oxygen compound. The distillate is perfectly white: it contains a little oil, which may again be pressed out. By crystallization out of pure æther, the melting-point may now be raised to 62° C. This substance was analysed.

0.261 grm. gave CO_2 0.8165 HO. 0.3393
giving in 100 parts,—

Carbon	85.31
Hydrogen	14.44
		<hr/> 99.75

The formula $\text{C}_m \text{H}_m$ demands—

C_m	85.71
H_m	14.28
		<hr/> 99.99

The difference between the hydrogen calculated and found is only 0.16 per cent., which is as near to theory as such analyses can be expected to come. Cerotene melts at 81° C. The hydrocarbon I have called cerotene melts at 57° to 58° . Melissine melts at 85° . The wax hydrocarbon at 62° C., showing a precisely analogous difference in their melting-points. Owing to the numerous operations which are neces-

sary before this hydrocarbon can be procured in a pure state, I have been unable to make further experiments with the pure substance. The analyses, however, the analogy of this other substance and the mode of its formation, can leave no doubt but that it is the hydrocarbon of the wax alcohol $C_{60}H_{60}$, to which may be given the name of melene.

The Nature of Myricine.

The analogy of the products of the decomposition of myricine by alkalies and by heat, to those of the Chinese wax and of spermaceti under similar circumstances, would lead us to suspect that a similar relation exists between the substances to which these products are due. If, however, we take the numbers which have been obtained by analysis for this body, those for example of Ettling*, or those of Lewy†, and attempt from these to reckon out a formula which shall give a rational account of these decompositions, we find a considerable deficiency of carbon. I give one of Lewy's analyses, with which other analyses of himself and other chemists are sufficiently accordant‡.

Carbon	80.28
Hydrogen	13.34
Oxygen	6.38
	<hr/>
	100.00

The formula $C_{92}H_{92}O_4$, which would account in a simple manner for the decompositions,—

$C_{32}H_{31}O_3$	$C_{32}H_{32}O_4$
$C_{60}H_{61}O$	$C_{60}H_{60}$
<hr/>	<hr/>
$C_{92}H_{92}O_4$	$C_{92}H_{92}O_4$

requires—

		Atomic weight.
C_{92} . .	81.65	552
H_{92} . .	13.60	92
O_4 . .	14.75	32
	<hr/>	<hr/>
	100.00	676

leaving a difference of one and a half per cent. of carbon, a difference too great to be attributed to any accidental error.

I have stated that the decompositions of the myricine are far from being so simple as those of the Chinese wax, and that in order to obtain either the acid or the wax alcohol, long and repeated crystallizations are necessary. This at once led

* Liebig's *Annalen*, vol. ii. p. 267.

† *Annales de Chimie*, vol. xiii. p. 443.

‡ Ibid.

me to the suspicion that the so-called myricine was no pure chemical substance, but a mixture of two or more bodies. Subsequent experiment confirmed this view.

The residue of the wax, after the cerotic acid has been boiled out by alcohol, melts at 64° C. It is but very slightly soluble in alcohol. Pure æther, however, will dissolve it without much difficulty. It crystallizes out of this reagent in light feathery crystals. The precipitate and the residue from the solution, evaporated to dryness, have different melting-points. I succeeded in this manner in raising the melting-point of the precipitate to $71^{\circ}5$. This end may be more readily obtained by adding a small quantity of naphtha to the æther.

The following analyses were made of a substance of 72° , which after repeated crystallizations was precipitated on the filter out of the hot solution, the filter being kept hot by means of a hot water apparatus. I have not succeeded in raising the melting-point beyond 72° . The substance is now highly crystalline in appearance, which the impure myricine is not, and of about the consistency of wax. I regard it in this state as pure.

	CO ₂ .	HO.
I*. 0.2592 grm. of substance gave	0.7735	0.3135
II. 0.2243 grm. of substance gave	0.672	0.269

which give in 100 parts,—

	I.	II.
Carbon . . .	81.38	81.70
Hydrogen . . .	13.44	13.33
Oxygen . . .	5.18	4.97
	<hr/> 100.00	<hr/> 100.00

These numbers are very different from any which have been before obtained for any substance from the myricine, and different from those which I myself have obtained for substances of a lower melting-point. The crystalline appearance marks the purity of the substance, and notwithstanding the slight difference in the hydrogen, I cannot but regard it as the body $C_{92}H_{92}O_4$, with the calculated formula of which, as given above, it sufficiently agrees. I must add that the substance is separable with extreme difficulty. The next precipitate

* The thorough combustion of these waxes is difficult, and I have made many experiments to ascertain the best method of analysis. Bichromate of lead was the material generally employed. But when the combustion is made very slowly, I believe it to be complete even with oxide of copper alone. The greater number of such analyses in this investigation were made by my chemical assistant, Mr. L. Hoffmann, to whose care and skill I am much indebted.

from the solution from which the above substance had been separated, had a melting-point half a degree lower, and gave to analysis rather less carbon, namely, C. $81^{\circ}0$ per cent.

The greater part of the difficultly saponifiable portion of the wax appears to consist of the substance, the analysis of which I have just given, and to which we may confine the name *myricine*. We have, however, clearly some other body present accompanying it, the products of the decomposition of which by potash are to be found with both the acid and the wax-alcohol procured by saponification of the impure substance, which, as I have said, render extremely difficult the preparation of these bodies in a pure state. I shall proceed to give some experiments which throw some true light upon the nature of this substance, although I cannot say that its history is satisfactorily made out. The solution of æther or naphtha (p. 247) from which the *melissine* of 85° has been separated, still contains a large quantity of substance of a similar appearance, but of a melting-point much lower than that of the *melissine* itself. Notwithstanding however the differences in the melting-point, analysis shows us but little or rather no difference in the constitution of the different portions of this substance. In the case for example of a substance melting at $78^{\circ}5$ C.,—

	CO ₂ .	HO.
0·2522 grm. gave . . .	0·764	0·324
which gives in 100 parts,—		
Carbon	82·59	
Hydrogen	14·27	
Oxygen	3·14	
	<hr/> 100·00	

In the case again of a substance melting at 72° ,—

	CO ₂ .	HO.
0·249 grm. gave . . .	0·75075	0·317
which gives in 100 parts,—		
Carbon	82·22	
Hydrogen	14·14	
Oxygen	3·64	
	<hr/> 100·00	

Other analyses gave similar results.

These analyses do not differ seriously from one another, and give precisely the numbers of the *melissine* itself (p. 247).

The numbers however are consistent with various formulæ besides that of the melissine. At 72° the melting-point is extremely constant. A portion of substance was obtained at this melting-point by repeatedly filtering the ætherial solution from the melissine which first crystallized out of the hot liquid. A time arrived when there was no difference between the melting-point of the portion which first crystallized out of the hot solution and which was on the filter, and that which afterwards crystallized out of the fluid which had passed through. The melting-point in both cases was 72° . By heating with lime and potash, as in the case of melissine, this substance of 72° also affords an acid, which after the usual preparation, gives very different numbers to those of the melissic acid. This acid melts at $77^{\circ}\cdot 5$.

		CO ₂ .	HO.
I.	0.256 grm. gave	0.735	0.3015
II.	0.267 grm. gave	0.765	0.311
III.	0.2551 grm. gave	0.730	0.2995

giving in 100 parts,—

	I.	II.	III.
Carbon . .	78.28	78.14	78.05
Hydrogen . .	13.09	12.94	13.05
Oxygen . .	8.63	8.92	8.90
	<u>100.00</u>	<u>100.00</u>	<u>100.00</u>

Between the second and third analyses the substance was twice crystallized out of æther. The substance dissolved by the æther had the same melting-point of 78° as the substance on the filter.

The silver salt of this acid gave the following numbers:—

	CO ₂ .	HO.
I. 0.5054 grm. of substance gave	1.127	0.4572
II. 0.5182 grm. of substance gave	1.1505	0.467

giving in 100 parts,—

	I.	II.
Carbon	60.80	60.56
Hydrogen	10.05	10.01
Oxygen and silver . .	29.15	29.43
	<u>100.00</u>	<u>100.00</u>

I. 0.617 grm. gave on ignition	0.1375 silver.
II. 0.7315 grm. gave on ignition	0.1625 silver.

giving per cent.—

	I.	II.
Silver . .	22.28	22.21
	S 2	

These analyses perfectly agree with the formulæ for the acid, $C_{49}H_{49}O_4$.

	Calculated.
C_{49} . . .	78·4
H_{49} . . .	13·0
O_4 . . .	8·6
	<hr/> 100·0

	Calculated.
C_{49} . . .	60·9
H_{48} . . .	9·9
O_4 . . .	6·8
Ag . . .	22·4
	<hr/> 100·0

If we compare the numbers of this acid with those of the substance from the oxidation of which it was derived, we shall see that it is impossible to account for the changes in the same simple manner as in other cases of such transformation. It would not be difficult to reckon out a formula that without great violence should account for it, but it is hardly worth while to do so, since notwithstanding the perfect agreement of the calculated and theoretical numbers, it is impossible to assert with certainty that either it or the body from which it is derived are pure chemical substances. There is too great a difficulty in the perfect separation of the melissine to lead us to hope that it can absolutely be removed by the method I have given. I failed in attempting to procure in larger quantities this substance of 72° . The melting-point was very constant at 75° , but on oxidizing a considerable quantity of this substance with lime and potash, acids were procured, which by crystallization were separable in the same manner as the substance from which they were derived, and the purification and perfect separation of which presented the same difficulties. I obtained in this way an acid having nearly the melting-point of 85° , the melting-point of melissic acid, and also an acid with a lower melting-point than 77° , but of which the melting-point was not so absolutely constant as to induce me to investigate it further. I give however these analyses, since they unquestionably prove the existence of some other body in addition to the melissine, in the products of the saponification of wax, which by oxidation is capable of passing into an acid belonging to the series $C_mH_mO_4$. Since it is only a pure body or a mixture of acids of this series which could give rise to the results I have given, and from the great difficulty of separation, the acid in all probability contains a very large

number of equivalents of carbon, whether it have precisely the formula I have above given or not.

Mixed with the palmitic acid of 62° , is found another acid of a much lower melting-point, and which presents similar difficulties of separation from the palmitic acid to those of the substance mixed with the melissine from the melissine itself. This acid is very soluble in alcohol, unctuous to the touch, and of a very low melting-point. I do not, however, mean to assert that the other wax-alcohol exists in the wax in combination with this unctuous acid, the presence of which is very probably due to another source.

This alcohol may possibly, as well as the melissine, be combined with palmitic acid, or it may be in some altogether different form in the wax. Even after long boiling with alcohol, the myricine has a slight wax smell, and it is possible that this unctuous acid is the product of the action of potash upon the oil which is one of the constituents of the wax, and from which I have in fact procured an acid of this nature. This oil, or rather grease, which was analysed by Lewy, is a very curious substance. The other constituents of the wax are, in a pure state, inodorous and crystalline, and to it the wax owes its tenacity and peculiar smell. I have made some experiments as to its nature, and procured from it also an acid and an unsaponifiable substance; I will not, however, here enter upon the matter, hoping at some future time to resume its investigation.

I must not omit to mention, with reference to the bees'-wax from Ceylon, of which I spoke in a former paper, and which contained no cerotic acid, that it possesses all the general characters of the other portion of the wax. Like the impure myricine, it contains more than one substance. The wax itself has a melting-point of $65^{\circ}.5$. When digested with æther in the cold, a portion is taken up by the æther, and a residue left of the melting-point of 67° ; and, when dissolved in æther, if the ætherial solution be filtered while warm from the first portions of the precipitate which crystallizes out, a substance may be obtained, of the melting-point of 72° , crystalline in appearance, hardly at all acted on by a solution of potash, but readily saponified by melted potash; resembling in short in all its properties the pure myricine. The products of the saponification of the wax itself closely resemble those of the impure myricine, and present similar difficulties of separation.

An acid may be obtained from it having the character of palmitic acid, and I have also procured from this wax the substance melissine, having a melting-point of 84° . The analysis VI. p. 247, was made from a preparation from Ceylon wax.

I will sum up the results of this investigation by giving a list of the principal substances of which an account has been given in this and the preceding papers. This table will exhibit, at one view, their relations to one another, and to the natural substances from the decomposition of which they are derived.

Cerotic acid [cerine] . . .	= $C_{54} H_{54} O_4$.
Chlor-cerotic acid . . .	= $C_{54} \left\{ \begin{matrix} H_{42} \\ Cl_{12} \end{matrix} O_4 \right.$.
Cerotic æther . . .	= $C_{58} H_{58} O_4 = \left\{ \begin{matrix} C_{54} H_{53} O_3 \\ C_4 H_5 O \end{matrix} \right.$.
Chlor-cerotic æther . . .	= $C_{58} \left\{ \begin{matrix} H_{46} O_4 \\ Cl_{12} \end{matrix} \right. = \left\{ \begin{matrix} C_{54} \left\{ \begin{matrix} H_{41} \\ Cl_{12} \end{matrix} O_3 \\ C_4 H_5 O \end{matrix} \right.$.
Cerotine . . .	= $C_{54} H_{56} O_2$.
Sulphate of oxide of cerotyle	= $SO_3, C_{54} H_{55} O + HO$.
Chlor-cerotal . . .	= $C_{54} \left\{ \begin{matrix} H_{41} \\ Cl_{13} \end{matrix} O_2 \right.$.
Cerotene [paraffine] . . .	= $C_{54} H_{54}$.
Chlor-cerotene . . .	= $\left\{ \begin{matrix} C_{54} \left\{ \begin{matrix} H_{35} \\ Cl_{19} \end{matrix} \right. \\ C_{54} \left\{ \begin{matrix} H_{33} \\ Cl_{21} \end{matrix} \right. \\ C_{54} \left\{ \begin{matrix} H_{32} \\ Cl_{22} \end{matrix} \right. \end{matrix} \right.$.
Chinese wax . . .	= $C_{108} H_{108} O_4 = \left\{ \begin{matrix} C_{54} H_{53} O_3 \\ C_{54} H_{55} O \end{matrix} \right.$.

Melissine . . .	= $C_{60} H_{62} O_2$.
Chlor-melal . . .	= $C_{60} \left\{ \begin{matrix} H_{45.5} \\ Cl_{14.5} \end{matrix} O_2 \right.$.
Melissic acid . . .	= $C_{60} H_{60} O_4$.
Melene [paraffine] . . .	= $C_{60} H_{60}$.
Palmitic acid . . .	= $C_{32} H_{32} O_4$.
Myricine (pure) . . .	= $C_{92} H_{92} O_4 = \left\{ \begin{matrix} C_{32} H_{31} O_3 \\ C_{60} H_{61} O \end{matrix} \right.$.

I might add to this list the acid $C_{49} H_{49} O_4$, the constitution of which however, for the reasons I have given, I cannot consider to be made out with sufficient certainty.

We should naturally suspect some intimate chemical relation between wax and fat from their similar appearance and properties. This suspicion gave rise to the idea that wax was convertible into fat, and to the hypothesis that wax was to be

regarded as the aldehyde of stearic acid, and was capable of passing into that substance by a simple process of oxidation, a view of its chemical nature entirely without foundation. From the preceding inquiry, we arrive however at the knowledge of a no less remarkable relation between these substances.

Margaric acid was recently the last of that singular series of acids of the type $C_m H_m O_4$, which commencing with formic acid comprehended acetic acid, the volatile acids of butter and the acid of spermaceti, and æthal was the last of the corresponding alcohols. In the wax acids and alcohols of which an account has been given in this and the preceding papers, we have bodies at the other extremity of the series standing in a similar relation to margaric acid and to æthal, as that in which acetic and butyric acid, and alcohol and potatoe oil stand to them at the commencement. An intervening acid of the series, the acid $C_{44} H_{44} O_4$, has lately been discovered by Völcker* in the oil of the *Guilandina Moringa*, and the investigation of the numerous class of vegetable oils and waxes will doubtless afford other bodies of the group.

Notwithstanding the many different properties of these substances, we find their chemical analogies constant, and the mutual relation of the acid, the alcohol and the hydrocarbon, is the same between bodies containing sixty as between those containing only four equivalents of carbon. Through at least half the series, from thirty to sixty equivalents, the same physical type of fat prevails. As a fat is doubtless but a soft kind of wax, so may not alcohol be but a very fluid form of fat? Alcohol has not yet been solidified, but one cannot help suspecting that when solidified it will appear as a wax or fat.

Direct experiment has shown us that in the body of the bee sugar is converted into wax. A simple analysis of the two substances showed that the carbon and hydrogen were in the same ratio in both, and that the change could be effected by a simple deoxidation of the sugar. Of the way in which this change is effected we are ignorant. The true formula of these wax substances however shows that they belong to the very type of bodies which are the ordinary products of fermentation, and are connected with them by the strongest chemical analogies. A new mode of fermentation produced butyric acid out of sugar; might not another kind of fermentation produce wax?

Until we know the nature of the whole of the ingredients of the wax, it is useless to speculate on the law of such a change. Although the wax itself is no pure chemical substance, but a mixture of substances differing nearly three per

* Liebig's *Annalen*, vol. lxiv. p. 342.

cent. from one another in their amount of carbon, yet the analysis of the whole bees'-wax gives results showing in different specimens which I have examined, no difference of constitution which analysis can reach. This renders it probable that the action is definite, and that the sugar in all cases loses the same amount of oxygen, although the remaining elements may in different cases be differently grouped.

XXXIV. On the Theory of the Tides.

By the Rev. BRICE BRONWIN.

[Continued from p. 192.]

WE are now prepared to find (ω) ; for we have

$$\frac{\delta\omega}{\delta\varpi} = \sin^2\theta \frac{d^2v}{dt^2} + 2n \sin\theta \cos\theta \frac{du}{dt} = \Sigma\{2n \cos\theta A - (n - cv)iB\} \\ \sin\theta \sin i(\varphi - \epsilon).$$

Or, by neglecting the very small term containing (v) , and then eliminating B ,

$$\frac{\delta\omega}{\delta\varpi} = \Sigma(2 \cos\theta A - iB)n \sin\theta \sin i(\varphi - \epsilon) = \Sigma\left(\cos\theta A - \sin\theta \frac{dA}{d\theta}\right) \\ n \sin\theta \sin i(\varphi - \epsilon) = \Sigma D \sin i(\varphi - \epsilon) \text{ suppose.}$$

Integrating, observing that $\varphi = nt + \varpi - \psi$, and $\psi + \epsilon =$ a constant, we have

$$\left. \begin{aligned} -\omega &= \Sigma \left\{ \frac{1}{i} D \cos i(\varphi - \epsilon) \right\}, \\ \text{or } -\omega &= \frac{1}{2} D_2 \cos 2(\varphi - \epsilon_2) + D_1 \cos(\varphi - \epsilon_1) \end{aligned} \right\} \quad (9.)$$

where we neglect the arbitrary, because we take account only of the terms containing φ .

When $i=2$,

$$\left(\cos\theta A - \sin\theta \frac{dA}{d\theta}\right) \sin\theta = -\sin^3\theta \frac{d}{d\theta} \left(\frac{A}{\sin\theta}\right).$$

But

$$\frac{A}{\sin\theta} = \frac{1}{2} a_2 \left(\frac{1}{\cos^4 \frac{\theta}{2}} + \frac{2}{\cos^2 \frac{\theta}{2}} \right), \\ \frac{d}{d\theta} \left(\frac{A}{\sin\theta} \right) = a_2 \sin \frac{\theta}{2} \left(\frac{1}{\cos^5 \frac{\theta}{2}} + \frac{1}{\cos^3 \frac{\theta}{2}} \right),$$

$$\begin{aligned}
 -\sin^3 \theta \frac{d}{d\theta} \left(\frac{A}{\sin \theta} \right) &= -8a_2 \sin^4 \frac{\theta}{2} \left(\frac{1}{\cos^2 \frac{\theta}{2}} + 1 \right) \\
 &= -4a_2 \sin^2 \frac{\theta}{2} \tan^2 \frac{\theta}{2} (3 + \cos \theta).
 \end{aligned}$$

When $i=1$,

$$\left(\cos \theta A - \sin \theta \frac{dA}{d\theta} \right) \sin \theta = \sin \theta \cos \theta A = a_1 \sin \theta \cos \theta.$$

Therefore

$$D_2 = -4na_2 \sin^2 \frac{\theta}{2} \tan^2 \frac{\theta}{2} (3 + \cos \theta), \quad D_1 = na_1 \sin \theta \cos \theta.$$

These results have been obtained by substituting the values of A from (8.); and it may be observed that all the functions of (θ) are discontinuous at the equator, and only hold from the pole to the equator. By suitably changing the arbitraries we may now write,

$$\left. \begin{aligned}
 -\frac{\omega}{g} &= D_2 \cos 2(\phi - \epsilon_2) + D_1 \cos(\phi - \epsilon_1) \\
 D_2 &= a_2 \sin^2 \frac{\theta}{2} \tan^2 \frac{\theta}{2} (3 + \cos \theta), \quad D_1 = a_1 \sin \theta \cos \theta.
 \end{aligned} \right\} \quad (10.)$$

If we make

$$\frac{a}{r} = \rho = 1 + e \cos(z - \pi),$$

where (a) is the mean distance, (e) the eccentricity, (z) the longitude, and (π) the long. of the apse of the planet; and if we also make

$$E_2 = \frac{3L}{4ga^3} \sin^2 \theta, \quad E_1 = \frac{3L}{ga^3} \sin \theta \cos \theta,$$

we have from (1.), retaining only the same terms as heretofore,

$$\frac{\alpha V'}{g} = E_2 \rho^3 \cos^2 v \cos 2\phi + E_1 \rho^3 \sin v \cos v \cos \phi.$$

For these terms, therefore,

$$y = \frac{\alpha V'}{g} - \frac{\omega}{g};$$

or by substitution,

$$\begin{aligned}
 y &= (D_2 \cos 2\epsilon_2 + E_2 \rho^3 \cos^2 v) \cos 2\phi + D_2 \sin 2\epsilon_2 \sin 2\phi \\
 &\quad + (D_1 \cos \epsilon_1 + E_1 \rho^3 \sin v \cos v) \cos \phi + D_1 \sin \epsilon_1 \sin \phi.
 \end{aligned}$$

Assume

$$y = F_2 \cos 2(\phi - \beta_2) + F_1 \cos(\phi - \beta_1); \quad . \quad . \quad (11.)$$

or

$$y = F_2 \cos 2\beta_2 \cos 2\phi + F_2 \sin 2\beta_2 \sin 2\phi + F_1 \cos \beta_1 \cos \phi + F_1 \sin \beta_1 \sin \phi.$$

This value of (y) (the height of the tide), compared with the preceding, gives

$$\left. \begin{aligned} F_2 \cos 2\beta_2 &= D_2 \cos 2\epsilon_2 + E_2 \rho^3 \cos^2 v, & F_2 \sin 2\beta_2 &= D_2 \sin 2\epsilon_2 \\ F_1 \cos \beta_1 &= D_1 \cos \epsilon_1 + E_1 \rho^3 \sin v \cos v, & F_1 \sin \beta_1 &= D_1 \sin \epsilon_1 \end{aligned} \right\} \quad (12.)$$

From these equations we must determine F_2 , F_1 , β_2 , and β_1 .

In small seas it is very easy to see that the direct effect of the force is very small compared with the horizontal velocities, and therefore that we ought to have D_2 much larger than E_2 . We shall proceed on this supposition. Therefore if $\beta_2 = \epsilon_2 + \Delta$, Δ is a very small quantity, and we shall have

$$\cos 2\beta_2 = \cos 2\epsilon_2 - 2\Delta \sin 2\epsilon_2, \quad \sin 2\beta_2 = \sin 2\epsilon_2 + 2\Delta \cos 2\epsilon_2$$

nearly by Taylor's theorem. Multiply the first of (12.) by the first of these, and the second by the second of these, member by member, and add the products; and we find

$$F_2 = D_2 + E_2 \rho^3 \cos^2 v \cos 2\epsilon_2; \quad . \quad . \quad . \quad (13.)$$

neglecting the small term

$$-2E_2 \Delta \rho^3 \cos^2 v \sin 2\epsilon_2,$$

which contains the product of the two small quantities E_2 and Δ .

Dividing the second of (12.) by (13.), member by member, there results

$$\sin 2\beta_2 = \sin 2\epsilon_2 - \frac{E_2}{D_2} \rho^3 \cos^2 v \sin 2\epsilon_2 \cos 2\epsilon_2$$

very nearly. If we put for $\sin 2\beta_2$ in this its value before given, we find

$$\Delta = -\frac{E_2}{2D_2} \rho^3 \cos^2 v \sin 2\epsilon_2,$$

and consequently

$$\beta_2 = \epsilon_2 - \frac{E_2}{2D_2} \rho^3 \cos^2 v \sin 2\epsilon_2. \quad . \quad . \quad . \quad (14.)$$

We cannot positively say that D_1 is large compared with E_1 ; but if we add together the squares of the third and fourth of (12.), we have

$$F_1^2 = D_1^2 + 2D_1 E_1 \rho^3 \sin v \cos v \cos \epsilon_1 + E_1^2 \rho^6 \sin^2 v \cos^2 v.$$

Suppose

$$F_1 = D_1 + E_1 \rho^3 \sin v \cos v \cos \epsilon_1. \quad . \quad . \quad . \quad (15.)$$

The square of the second member of this will differ from the

second member of the preceding only by the small quantity $E_1^2 \rho^6 \sin^2 v \cos^2 v \sin^2 \epsilon_1$. We cannot affirm that this is a very near approximation; and without more knowledge of the comparative values of D_1 and E_1 , we cannot express β_1 as we have done β_2 . Dividing the third of (12.) by the fourth, we have

$$\cot \beta_1 = \cot \epsilon_1 + E_1 \rho^3 \frac{\sin v \cos v}{\sin \epsilon_1}; \quad . \quad . \quad . \quad (16.)$$

whence it appears that β_1 may in some cases vary very considerably.

Let (o) be the angle which the orbit of the planet makes with the equator, (z) the longitude measured on it from their intersection. Then (o) is the obliquity and (z) the true longitude for the sun, and they are nearly the same for the moon. Also (z) is the hypotenuse, (v) the perpendicular, and (o) the angle at the base of a right-angled spherical triangle; and (ψ) is the base for the sun, and nearly so for the moon. Hence by spherical trigonometry,

$$\tan \psi = \cos o \tan z,$$

and therefore

$$\frac{d\psi}{\cos^2 \psi} = \cos o \frac{dz}{\cos^2 z}, \quad \frac{d\psi}{dz} = \cos o \frac{\cos^2 \psi}{\cos^2 z}$$

But

$$\cos z = \cos v \cos \psi,$$

consequently

$$\frac{d\psi}{dz} = \frac{\cos o}{\cos^2 v} = \cos o (1 + \sin^2 v),$$

neglecting $\sin^4 v$ and higher powers. Also

$$\sin v = \sin o \sin z;$$

therefore

$$\begin{aligned} \frac{d\psi}{dz} &= \cos o (1 + \sin^2 o \sin^2 z) = \cos o \left(1 + \frac{1}{2} \sin^2 o - \frac{1}{2} \sin^2 o \cos 2z \right) \\ &= 1 - \frac{1}{2} \sin^2 o \cos 2z, \end{aligned}$$

neglecting $\sin^4 o$, &c.

But

$$vt + \epsilon = z - 2e \sin (z - \pi);$$

consequently

$$v dt = dz - 2e dz \cos (z - \pi),$$

neglecting e^2 , &c.

It may be doubtful whether we should express the value of ϵ in terms of the mean longitude or of the true; I have chosen

the latter. By means of the above formulæ we have

$$d\mathcal{E} = c v dt - d\psi = dz \left\{ c - 1 + \frac{1}{2} \sin^2 o \cos 2z - 2ec \cos (z - \pi) \right\}.$$

As (\mathcal{E}) cannot contain a term increasing with z or t , we must have $c=1$; then

$$d\mathcal{E} = dz \left\{ \frac{1}{2} \sin^2 o \cos 2z - 2e \cos (z - \pi) \right\},$$

and

$$\mathcal{E} = k + \frac{1}{4} \sin^2 o \sin 2z - 2e \sin (z - \pi). \quad . \quad . \quad (17.)$$

Change (k) into (k_2) and (k_1), and we have the values of (\mathcal{E}_2) and (\mathcal{E}_1). Whence by Taylor's theorem,

$$\sin 2\mathcal{E}_2 = \sin 2k_2 + \frac{1}{2} \sin^2 o \cos 2k_2 \sin 2z - 4e \cos 2k_2 \sin (z - \pi),$$

$$\cos 2\mathcal{E}_2 = \cos 2k_2 - \frac{1}{2} \sin^2 o \sin 2k_2 \sin 2z + 4e \sin 2k_2 \sin (z - \pi)$$

nearly.

Putting for ρ^3 its value in (13.) and (14.), and $1 - \sin^2 v$ for $\cos^2 v$, and neglecting the very small terms containing $e \sin^2 v$, we have

$$F_2 = D_2 + E_2 \cos 2\mathcal{E}_2 - E_2 \sin^2 v \cos 2\mathcal{E}_2 + 3e E_2 \cos 2\mathcal{E}_2 \cos (z - \pi),$$

$$\beta_2 = \mathcal{E}_2 - \frac{E_2}{2D_2} \sin 2\mathcal{E}_2 + \frac{E_2}{2D_2} \sin^2 v \sin 2\mathcal{E}_2 - \frac{3e E_2}{2D_2} \sin 2\mathcal{E}_2 \cos (z - \pi).$$

Substituting for \mathcal{E}_2 its value from (17.) in these, and neglecting some very small quantities, we find, making to abridge,

$$K_2 = D_2 + E_2 \cos 2k_2, \quad G_2 = k_2 - \frac{E_2}{2D_2} \sin 2k_2,$$

$$H_2 = 1 - \frac{E_2}{D_2} \cos 2k_2,$$

$$F_2 = K_2 - E_2 \cos 2k_2 \sin^2 v - \frac{1}{2} E_2 \sin^2 o \sin 2k_2 \sin 2z$$

$$+ 3e E_2 \cos 2k_2 \cos (z - \pi) + 4e E_2 \sin 2k_2 \sin (z - \pi)$$

$$\beta_2 = G_2 + \frac{E_2}{2D_2} \sin 2k_2 \sin^2 v - \frac{1}{4} H_2 \sin^2 o \sin 2z - 2e H_2 \sin (z - \pi)$$

$$- \frac{3e E_2}{2D_2} \sin 2k_2 \cos (z - \pi). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

Since

$$\sin o \sin 2z = 2 \sin o \sin z \cos z = 2 \sin v \cos z = 2 \sin v \cos \psi$$

nearly, these may be replaced by

$$F_2 = K_2 - E_2 \cos 2k_2 \sin^2 v - E_2 \sin o \sin 2k_2 \sin v \cos \psi \\ + 3eE_2 \cos 2k_2 \cos (z - \pi) + 4eE_2 \sin 2k_2 \sin (z - \pi),$$

$$\beta_2 = G_2 + \frac{E_2}{2D_2} \sin 2k_2 \sin^2 v - \frac{1}{2} H_2 \sin o \sin v \cos \psi \\ - 2eH_2 \sin (z - \pi) - \frac{3eE_2}{2D_2} \sin 2k_2 \cos (z - \pi).$$

Or, if we please, since

$$\sin^2 v = \sin^2 o \sin^2 z = \frac{1}{2} \sin^2 o - \frac{1}{2} \sin^2 o \cos 2z,$$

$$F_2 = L_2 + \frac{1}{2} E_2 \sin^2 o \cos 2(z + k_2) + 3eE_2 \cos (z - 2k_2 - \pi)$$

nearly,

$$= L_2 + \frac{1}{2} E_2 \sin^2 o \cos 2(\psi + k_2) + 3eE_2 \cos (\psi - 2k_2 - \pi)$$

nearly,

$$\beta_2 = G_2 + \frac{E_2}{2D_2} \sin 2k_2 \sin^2 v - \frac{1}{2} H_2 \sin o \sin v \cos \psi - 2eH_2 \\ \sin (\psi - \pi)$$

nearly by neglecting the last term, which is very small, as also are some of those terms containing this quantity which have been retained. In the above

$$L_2 = K_2 - \frac{1}{2} E_2 \sin^2 o \cos 2k_2 = D_2 + E_2 \cos o \cos 2k_2$$

nearly.

Making $\rho = 1$, and $\epsilon_1 = k_1$, in the small terms, we may make

$$F_1 = D_1 + E_1 \cos k_1 \sin v \cos v.$$

But this will not be very near the truth unless D_1 be something larger than E_1 ; and we cannot conveniently express β_1 . But if D_1 be considerably larger than E_1 , we have

$$\sin \beta_1 = \sin \epsilon_1 \left(1 - \frac{E_1}{D_1} \cos k_1 \sin v \cos v \right)$$

nearly. Also

$$\sin \epsilon_1 = \sin k_1 + \frac{1}{4} \sin^2 o \cos k_1 \sin 2z$$

nearly, neglecting the term containing e . Hence we easily find

$$\sin \beta_1 = \sin k_1 - \frac{E_1}{D_1} \sin k_1 \cos k_1 \sin v \cos v + \frac{1}{4} \sin^2 o \cos k_1 \sin 2z$$

$$= \sin k_1 - \frac{E_1}{D_1} \sin k_1 \cos k_1 \sin v \cos v + \frac{1}{2} \sin o \cos k_1 \sin v \cos \psi$$

nearly, and therefore

$$\beta_1 = k_1 - \frac{E_1}{D_1} \sin k_1 \sin v \cos v + \frac{1}{2} \sin o \sin v \cos \psi$$

nearly.

We might find in the value of $-\frac{\omega}{g}$ the terms $D_3 \cos 3(\phi - \epsilon_3)$ and $D_4 \cos 4(\phi - \epsilon_4)$; the coefficients D_3 and D_4 being, as before, functions of θ without t or z ; but these terms must be very small.

From (1.) it is easy to see that there will also be terms of the form

$l + m \sin^2 o \sin 2z + p \sin^2 o \cos 2z + qe \sin(z - \pi) + se \cos(z - \pi)$, the coefficients l, m , &c. being functions of θ ; but as they are very small, we shall not attempt to determine them. If, however, we wish to make $\delta\omega$ a complete variation relative to such terms, we must make the coefficient of $\delta\omega$ to vanish, or

$$\sin^2 \theta \frac{d^2 v}{dt^2} + 2n \sin \theta \cos \theta \frac{du}{dt} = 0,$$

which will give an equation of condition. The arbitrary of integration will now be a constant independent of θ . And we shall have

$$\delta\omega = \delta\theta \left(\frac{d^2 u}{dt^2} - 2n \sin \theta \cos \theta \frac{dv}{dt} \right),$$

or

$$\delta\omega = \delta\theta r^2 \left(\frac{d^2 u}{dt^2} - 2n \sin \theta \cos \theta \frac{dv}{dt} \right) - 2nr \delta r \sin^2 \theta \frac{dv}{dt},$$

according as we make $\delta\omega$ a complete variation relative to θ only (which, indeed, it is already) or relative to both θ and r . The equation of continuity will become

$$\sin \theta \frac{du}{d\theta} + \cos \theta u = 0,$$

or

$$\frac{d(r^2 s)}{dr} + r^2 \left(\frac{du}{d\theta} + u \cot \theta \right) = 0,$$

as the case may be. I believe we must complete the variation for both r and θ .

Gunthwaite Hall, near Barnsley, Yorkshire,

September 7, 1849.

[To be continued.]

XXXV. *On the Inorganic Constituents of Organic Bodies.* By
H. ROSE, *Professor of Chemistry in the University of Berlin.*

[Continued from p. 187.]

APPENDIX IX.

Examination of the Inorganic Constituents of the Flesh of the Horse. By M. Weber.

THE flesh consisted of the muscles of the fore-leg of a lean horse, immediately after the animal was killed, and completely freed from blood by the injection of water into the brachial artery until it escaped from the veins in a colourless state; it was then dried and carbonized.

The residue of the *aqueous extract* was perfectly free from carbonic acid, and consisted of—

Chloride of sodium	3·43	} or {	Na Cl . . .	3·43
Potash	48·19		2KO + PO ₅	83·27
Soda	5·18		2NaO + PO ₅	11·10
Phosphoric acid . .	41·68		KO, SO ₃ . .	1·52
Sulphuric acid . . .	0·71			<hr/> 99·32
	<hr/> 99·19			

The *muriatic extract* consisted of—

Potash	26·47
Soda	4·36
Lime	6·02
Magnesia	12·20
Peroxide of iron . . .	3·96
Phosphoric acid . . .	46·99
	<hr/> 100·00

Assuming that the phosphoric acid forms pyrophosphates with the bases, we obtain the following calculated result:—

Bibasic phosphate of lime . .	13·64
Bibasic phosphate of magnesia	33·27
2FeO ₂ + 3PO ₅	1·22
Bibasic phosphate of potash . .	30·14
Potash	9·28
Soda	4·45
	<hr/> 100·00

If, however, we admit that both the alkalies and the earths, excluding the magnesia, form *c*-phosphates, we obtain the following result:—

Tribasic phosphate of potash	39.82
Tribasic phosphate of soda	7.66
Tribasic phosphate of lime	11.35
Bibasic phosphate of magnesia	33.27
Perphosphate of iron	7.50
	<hr/> 99.60

The amount of phosphoric acid required by calculation is 46.61 per cent.; that found by analysis amounts to 46.99 per cent.

Residuary carbonized mass.—The ash of this consisted of—

Potash	36.64
Soda	4.71
Lime	1.88
Magnesia	4.36
Peroxide of iron	0.76
Phosphoric acid	51.65
	<hr/> 100.00

The precipitate thrown down by ammonia from the muriatic solution of this ash, after having been heated to redness, consisted of 2CaO , $\text{PO}_5 + 2\text{MgO}$, $\text{PO}_5 + \text{Fe}_2\text{O}_3$. The phosphoric acid required by this formula amounts to 9.87 per cent.; analysis gave exactly this quantity. The remaining 41.79 per cent. of phosphoric acid were neither wholly combined with the alkalis as pyrophosphates, nor as metaphosphates. The phosphoric acid is too large in quantity for the former case, and too small for the latter.

The relative amounts of ash in the flesh were as follows:—

Extracted by water	42.81
Extracted by muriatic acid	17.48
Ash of the remaining carbonaceous mass	39.71
	<hr/> 100.00

The amount of the whole of the inorganic constituents of the flesh was—

	Oxygen.	
Chloride of sodium	1.47	
Potash	39.95	6.77
Soda	4.86	1.24
Lime	1.80	0.50
Magnesia	3.88	1.50
Peroxide of iron	1.00	0.30
Phosphoric acid	46.74	26.19
Sulphuric acid	0.30	1.17
	<hr/> 100.00	

10.31

26.36

The proportion of oxygen in the bases to that of the phosphoric acid was as 2 : 5, *i. e.* the salts were pyrophosphates; and in this respect this ash has some analogy with that of wheat, which, however, contains far more alkaline chlorides.

Thus water and muriatic acid principally extract alkaline phosphates from carbonized flesh, and alkaline chlorides and carbonates from carbonized blood. The muriatic extract of the blood contains altogether so few constituents, that we may suppose they are only those which had previously resisted the solvent action of water, or were formed from the anoxidic portion of the blood by the imperfect exclusion of the air during its carbonization.

If so, and we admit that the alkaline chlorides and carbonates do not belong to those constituents of the blood, the inorganic portions of which consist of phosphates in an oxidized and unoxidized state, these constituents of the blood would contain the phosphates in a perfectly deoxidized state. Such are probably the proteine substances of the blood, which would then differ from those of the flesh, by the former being anoxidic and the latter meroxidic substances. Probably in future we must only call those substances anoxidic, meroxidic, and teleoxidic, the inorganic constituents of which consist principally of phosphates in a deoxidized, partly oxidized, and perfectly oxidized state. The blood will then be an anoxidic, and the flesh a meroxidic substance.

APPENDIX X. and XI.

Analysis of the Ashes of Human Fæces and Urine. By M. Fleitmann.

Although a single analysis of the ash of the fæces, without regard to the diet, can be of but little physiological importance, inasmuch as it must depend greatly upon the nature of the food consumed and upon the mode of life of the individual, yet a comparative examination of the inorganic constituents of the fæces and urine may afford us instructive conclusions regarding the quantities excreted in the same period of time. Such a comparison had not previously, I believe, been made, and as we shall see, has yielded a remarkable result. For this purpose the fæces and urine of a young man, aged 20, were carefully collected during four days. During this period his diet was very moderate, consisting principally of meat, and as little vegetable matter as possible. He drank no spirituous liquids, and little liquid of any kind during the period, but he took much corporeal exercise.

Fæces.—When dried at 212° F. they weighed only 104·10 grms. They were carbonized as usual.

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Aqueous extract.—On evaporation to dryness it left a residue of 1·933 grm., consisting of—

Chloride of sodium	3·15
Chloride of potassium	0·37
Potash	27·81
Hydrate of potash	54·18
Phosphoric acid	6·75
Sulphuric acid	1·57
Silica	0·52
Carbonic acid	5·65
	<hr/> 100·00

The large amount of hydrate of potash was produced by the action of the carbon upon the alkaline carbonate. These constituents correspond to the following salts:—

Chloride of sodium	3·15
Chloride of potassium	0·37
Tribasic phosphate of potash	20·13
Sulphate of potash	3·41
Silicate of potash	1·05
Carbonate of potash	17·71
Hydrate of potash	54·18
	<hr/> 100·00

Muriatic extract.—This left 6·493 grms. of residue, consisting of—

Potash	10·22	} or {	3CaO + PO ₅	56·98
Soda	1·06		3KO + PO ₅	15·38
Lime	31·32		3NaO + PO ₅	1·87
Magnesia	13·98		3MgO + PO ₅	18·30
Phosphoric acid	41·69		CaO, SO ₃	0·31
Sulphuric acid	0·18		CaO, SiO ₃	0·36
Silica	0·23		MgO	5·48
Peroxide of iron	1·32		Fe ₂ O ₃	1·32
	<hr/> 100·00			<hr/> 100·00

The magnesia must have existed in the carbonized mass in the form of carbonate.

Residuary carbonaceous mass.—This was considerable, and consisted principally of sand, part of which existed as such in the fæces, and even in the food; part must have been swallowed in the form of dust during the exercise taken by the subject of the experiment in the fields near Berlin. The residue weighed 1·996 grm., and consisted of—

Potash	4·83	} or {	$3\text{KO} + \text{PO}_5$	7·25
Soda	0·42		$3\text{NaO} + \text{PO}_5$	0·77
Lime	9·66		$3\text{CaO} + \text{PO}_5$	12·78
Magnesia	10·24		$3\text{MgO} + \text{PO}_5$	20·66
Peroxide of iron	6·61		CaO, SO_3	6·45
Phosphoric acid	19·61		MgO	0·62
Sulphuric acid	3·77		Fe_2O_3	6·61
Silica	6·25		SiO_3	6·25
Sand	38·61		Sand	38·61
<hr/>				
100·00				100·00

Hence the phosphates of the excrements are *c*-phosphates, and the bases are all in the proportion of three atoms to one of phosphoric acid. In the aqueous extract, the greater part of the potash is either combined with carbonic acid, or exists in the form of potash; whilst in the excrements themselves, the alkali was combined with an organic substance, which occupied the place of an acid. Since the fæces principally carry off those oxidized salts which are insoluble in water, whilst the urine removes those which are soluble in water, most of the inorganic constituents of the fæces are contained in the muriatic extract of the carbonized mass. The large quantity of phosphate of magnesia in this ash is remarkable.

The excrements might be regarded as teleoxidic substances; at least the unoxidized inorganic matters existing in them are so small, that they probably arise merely from the undigested remains of the food. The small quantity of soda present, compared with that of the potash, is also remarkable; especially as the bile principally contains soda, and but little potash. Hence the soda of the bile must be removed by the urine, not the fæces. The following are the inorganic constituents of the fæces as obtained by the three operations:—

Chloride of potassium	0·07	} or {	K Cl	0·07
Chloride of sodium	0·58		Na Cl	0·58
Potash	12·44		$3\text{KO} + \text{PO}_5$	14·70
Hydrate of potash	10·05		$3\text{NaO} + \text{PO}_5$	1·32
Soda	0·75		$3\text{CaO} + \text{PO}_5$	37·95
Lime	21·36		$3\text{MgO} + \text{PO}_5$	15·36
Magnesia	10·67		KO, SO_3	0·63
Peroxide of iron	2·09		KO, SiO_3	0·20
Phosphoric acid	30·98		CaO, SO_3	1·43
Sulphuric acid	1·13		CaO, SiO_3	0·23
Silica	1·44		KO, CO_2	3·28
Carbonic acid	1·05		KO, HO	10·05
Sand	7·39		$\text{Fe}_2\text{O}_3, \text{SiO}_3?$	3·28
			MgO	3·53
			Sand	7·39
<hr/>				
10·000				100·00

Examination of the Urine.—As the residue of the evaporated urine was very difficult to dry at 212° F., it was carbonized at once.

Aqueous extract.—This contained by far the greater part of the inorganic constituents of the urine. In the urine excreted during four days, it amounted to no less than 54.148 grms. These consisted of—

Chloride of sodium	62.78	} or of {	Na Cl	62.78
Chloride of potassium	9.89		K Cl	9.89
Potash	15.40		KO, SO ₃	5.87
Magnesia	0.32		2KO + PO ₅	16.12
Phosphoric acid	8.92		3KO + PO ₅	4.55
Sulphuric acid	2.69		2MgO + PO ₅	0.42
	100.00		3MgO + PO ₅	0.37
				100.00

Muriatic extract.—It amounted to 5.085 grms., and consisted of—

Soda	19.22	} or of {	3NaO + PO ₅	33.83
Potash	2.96		3KO + PO ₅	4.45
Lime	17.66		3CaO + PO ₅	29.99
Magnesia	13.65		3MgO + PO ₅	21.98
Phosphoric acid	41.51		CaO, SO ₃	3.18
Sulphuric acid	1.86		MgO, SiO ₃	6.19
Silica	2.76		Fe ₂ O ₃	0.38
Peroxide of iron	0.38			100.00
	100.00			

Residuary carbonaceous mass.—This left a very small quantity of ash on incineration, only 0.352 grm., the principal component of which was silica, weighing 0.156 grm.; the remainder consisted almost entirely of phosphate of magnesia. It has been suggested above, that this small quantity of inorganic constituents existed in the urine in a perfectly oxidized state, and had resisted the solvent action of the muriatic acid, probably because the magnesia had formed with the silica a compound insoluble in dilute muriatic acid.

If this view be adopted, all the inorganic constituents exist in the urine in a perfectly oxidized state; hence it is a perfectly teleoxidic substance.

The following are therefore the inorganic constituents of the carbonized mass of the evaporated urine :—

Chloride of sodium	57·03
Chloride of potassium	8·99
Tribasic phosphate of soda	2·90
Tribasic phosphate of potash	4·53
Bibasic phosphate of potash	4·65
Tribasic phosphate of lime	2·57
Tribasic phosphate of magnesia	2·57
Bibasic phosphate of magnesia	0·37
Sulphate of potash	5·33
Sulphate of lime	0·27
Magnesia, peroxide of iron, and silica	0·79
	<hr/> 100·00

These inorganic constituents must not be compared with those existing in the urine before carbonization. In the latter the bases are partly combined with organic acids, which are converted into carbonates during the process of carbonization, the carbonic acid of which is expelled by the phosphoric acid of the bibasic phosphates. Thus tribasic phosphates are formed, which cannot exist as such in the urine, because the latter exerts an acid reaction.

During the four days in which the fæces and urine were collected, the inorganic constituents of the former amounted to 10·422 grms., and the latter to 59·585. This remarkable result would not have been expected *à priori*. The difference becomes still more striking when the amount of the sand is deducted from the inorganic constituents of the fæces, and which can only be regarded as an accidental mixture.

The following comparison exhibits the inorganic components of the fæces and urine excreted in a day, excluding the sand:—

	Urine.	Fæces.
Chloride of sodium	8·9243 grms.	0·0167 grms.
Chloride of potassium	0·7511
Soda	0·0185 ...
Potash	2·4823 ...	0·5455 ...
Lime	0·2245 ...	0·5566 ...
Magnesia	0·2415 ...	0·2781 ...
Peroxide of iron . .	0·0048 ...	0·0544 ...
Phosphoric acid . .	1·7598 ...	0·8072 ...
Sulphuric acid . .	0·3864 ...	0·0293 ...
Silica	0·0691 ...	0·0375 ...
	<hr/> 14·8438	<hr/> 2·3438

Hence the amount of inorganic constituents in the urine is more than $6\frac{1}{2}$ times greater than that in the solid excrements.

The following are the weights of the inorganic constituents obtained in the different parts of the examination of the fæces and the urine:—

	Fæces.	Urine.
Extracted by water	18·55	90·87
Extracted by muriatic acid	62·30	8·54
In the ash of the residuary carbonized mass	19·15	0·59
	<u>100·00</u>	<u>100·00</u>

APPENDIX XII.

Examination of the Inorganic Constituents of the Bile (of Oxen). By M. Weidenbusch.

Aqueous extract of the carbonized mass.—This, when evaporated to dryness, consisted of—

Chloride of sodium	28·77	} or of {	Na Cl	28·77
Potash	4·51		3NaO + PO ₅	14·51
Soda	35·79		3KO + PO ₅	6·78
Phosphoric acid	8·55		NaO, SO ₃	8·55
Sulphuric acid	4·81		NaO, CO ₂	28·27
Carbonic acid	11·70		NaO, HO	9·34
Silica	0·26		Si O ₃	0·26
	<u>94·39</u>			<u>96·48</u>

Muriatic extract.—This consisted of—

Potash	3·70	} or of {	3KO + PO ₅	5·56
Soda	11·50		3NaO + PO ₅	20·25
Lime	27·00		3CaO + PO ₅	49·81
Magnesia	7·41		3MgO + PO ₅	14·71
Peroxide of iron	4·21		MgO	0·69
Manganoso-manganic oxide	2·11		F ₂ O ₃	4·20
Phosphoric acid	41·63		MnO, Mn ₂ O ₃	2·11
Silica	2·41		Si O ₃	2·41
	<u>99·97</u>			<u>100·00</u>

Residuary carbonaceous mass.—The ash consisted of—

Potash	6·71
Soda	40·49
Lime	2·45
Magnesia	4·01
Peroxide of iron	0·80
Phosphoric acid	3·89
Sulphuric acid	41·63
	<u>99·98</u>

The sulphur may be considered as existing in the carbonized bile, after exhaustion by the solvents, as a constituent of certain compound radicals, in the same manner as was assumed to be the case with the phosphorus in the carbonized product of other organic substances. But in this carbonized mass the amount of sulphur is much larger than would be found by calculation from that of the sulphuric acid obtained. A very large portion of it is volatilized during the oxidation. If the exhausted carbonized mass be mixed with nitrate of baryta, and the mixture be heated to redness, so much sulphate of baryta is obtained, that the quantity of sulphuric acid existing in it amounts to 30 per cent. more than that obtained by the mere oxidation of the carbonized mass.

The following are the proportions of the inorganic components of the bile as obtained in the three operations:—

Extracted by water	90.85
Extracted by muriatic acid	4.93
In the ash of the residue of the carbonaceous mass	4.22
	<hr/> 100.00

The following are the whole of the inorganic constituents of the bile of the ox:—

		Oxygen.	
Chloride of potassium	27.70		
Potash	4.80	0.81	} 10.90
Soda	36.73	9.39	
Lime	1.43	0.40	
Magnesia	0.53	0.20	
Peroxide of iron	0.23	0.07	
Manganoso-manganic oxide	0.12	0.03	} 17.99
Phosphoric acid	10.45	5.85	
Sulphuric acid	6.39	3.82	
Carbonic acid	11.26	8.14	
Silica	0.36	0.18	
	<hr/> 100.00		

The quantities of the acids are not correct, because, as we have stated, a far larger amount of sulphuric acid would have been obtained had the whole of the sulphur been converted into sulphuric acid.

APPENDIX XIII.

Examination of the Inorganic Constituents of Cow's Milk. By M. Weber.

The cows from which the milk was procured were fed with the refuse of a brewery in addition to the ordinary stall-fodder.

The milk was not skimmed, but evaporated at once and carbonized.

Aqueous extract.—The washing required to be continued for an extraordinary length of time. The residue of the evaporated extract consisted of—

Chloride of potassium	41·42	} or of {	KCl . . .	41·42
Chloride of sodium . .	13·85		Na Cl . . .	13·85
Potash	29·66		3KO + PO ₅	21·60
Phosphoric acid . . .	7·25		KO, SO ₃ . .	0·36
Sulphuric acid	0·17		KO, CO ₂ . .	22·83
Carbonic acid	7·27			<hr/> 100·06
	<hr/> 99·62			

Muriatic extract.—No evolution of carbonic acid could be perceived on the addition of the muriatic acid. The constituents were—

Potash	6·29
Soda	12·19
Lime	36·70
Magnesia	3·26
Peroxide of iron . .	0·30
Phosphoric acid . .	41·26
	<hr/> 100·00

Hence the muriatic acid had only dissolved phosphates.

Residuary carbonaceous mass.—It yielded—

Potash	33·13
Soda	9·01
Lime	16·58
Magnesia	3·40
Peroxide of iron . .	1·10
Phosphoric acid . .	36·60
Silica	0·18
	<hr/> 100·00

Thus the anoxidic portion of the milk, after oxidation, was of the same composition as the teleoxidic.

The following are the results of the examination of the milk:—

Extracted by water	34·17
Extracted by muriatic acid	31·75
Ash of the remaining carbonaceous mass . . .	34·08
	<hr/> 100·00

The whole of the constituents were—

		Oxygen.	
Chloride of potassium . . .	14.18		
Chloride of sodium . . .	4.74		
Potash	23.46	3.97	} 11.51
Soda	6.96	1.78	
Lime	17.34	4.87	
Magnesia	2.20	0.85	
Peroxide of iron	0.47	0.14	} 17.51
Phosphoric acid	28.04	15.71	
Sulphuric acid	0.05	0.02	
Carbonic acid	2.50	1.80	
Silica	0.06	0.03	
	100.00		

Hence the phosphoric acid of the bases is to that of the acids nearly as 3 : 5. The teleoxidic portion of the milk contains *c*-phosphates, and the anoxidic portion yields by oxidation *c*-phosphates also.

Thus milk is a meroxidic substance. We might almost call it a hemioxidic substance, if the large quantity of the alkaline chlorides contained in the aqueous extract were excluded from the teleoxidic portion, to which they evidently do not belong.

The large amount of phosphoric acid in the teleoxidic portion, and the considerable quantity which the anoxidic portion yields on oxidation, are remarkable. It is hence evident, as has frequently been remarked, how well the milk is adapted for effecting the ossification of the bones in the mammalia.

Whilst in the blood the bases predominate over the acids, in the flesh we find little else than pyrophosphates, and in the milk the bases for the most part form *c*-phosphates.

APPENDIX XIV. and XV.

Examination of the Inorganic Constituents of the White and Yolk of Hen's Eggs. By M. Poleck.

These experiments were among the first made by the method of carbonization, and were instituted before the process was perfected ; they do not therefore deserve too much confidence, although performed with great care. But as they appeared to me of some importance, I shall briefly describe them.

The principal source of error consists in the fact, that in some of the analyses the alkali contained in the muriatic extract, not having been suspected to exist there, was overlooked ; moreover, the exhausted carbonized mass being

burnt in an atmosphere of oxygen, would allow of the volatilization of a considerable portion of the alkaline phosphates. The separation of the white from the yolk can be easily effected, by well boiling the eggs in water until they become hard.

The relative proportion of the white and yolk was not exactly the same in all the eggs. The following results were obtained in regard to this point:—

Four eggs yielded	. 60.60 per cent. white
... ..	39.40 ... yolk
Sixteen eggs yielded	. 58.43 ... white
... ..	41.57 ... yolk
Fourteen eggs yielded	. 59.42 ... white
... ..	40.58 ... yolk.

White of egg.—It yielded in two instances—

	I.	II.
Chloride of potassium . .	47.19	51.33
Chloride of sodium . .	10.66	17.13
Soda	24.22	17.71
Sulphuric acid	1.61	1.67
Carbonic acid	14.66	10.49
Silica	0.17	
	<hr/> 98.51	<hr/> 98.53

or

Chloride of potassium . .	47.19	51.33
Chloride of sodium . .	10.66	17.13
Carbonate of soda . . .	39.23	28.01
Sulphate of soda	2.83	2.96
Silica	0.17	
	<hr/> 100.08	<hr/> 99.43

In both analyses a little more carbonic acid was found than could be combined with the alkali. This is remarkable; because in the aqueous extracts of the carbonized mass of other organic substances, considerably less carbonic acid was frequently found than was requisite for the saturation of the alkali, a considerable portion of the carbonic acid being frequently reduced to carbonic oxide by the carbon.

Muriatic extract.—As in the first experiment, the presence of the alkalies was overlooked; the result of the second only is given:—

Potash	4.95
Soda	9.13
Lime	10.53
Magnesia	11.61
Carbonate of lime	11.14
Carbonate of magnesia	15.48
Peroxide of iron	2.75
Phosphoric acid	23.85
Silica	10.56
	<hr/>
	100.00

Residuary carbonaceous mass.—It was not incinerated with platinum, but in oxygen gas; hence there was a loss. I have already remarked that the proteine substances of vegetables and animals alone appear to be meroxidic bodies; all others appear to be of a teleoxidic nature. The white of hen's eggs, however, forms a remarkable exception to all the other proteine substances which have been examined, in consequence of the very small quantity of anoxidic substance which it contains. The amount of ash is very small. In both experiments the charred mass contained silica in the form of sand, which, however, was deducted from the ash. The following was the composition of the two ashes:—

	I.	II.
Potash	11.93	16.76
Soda	10.83	5.48
Lime	12.21	8.21
Magnesia	24.15	9.02
Peroxide of iron	1.41	5.64
Phosphoric acid	30.73	37.24
Silica	8.71	17.63
	<hr/>	<hr/>
	99.94	99.98

These results differ very considerably; the cause must be determined by future experiments.

On arranging the constituents in the form of salts, we find in the first experiment *b*-phosphoric acid and some very basic silicates; in the second only *b*-phosphoric acid and less basic silicates.

The quantities obtained were—

	I.	II.
Extracted by water	81.52	82.19
Extracted by muriatic acid	14.33	15.52
In the ash of the remaining mass	4.15	2.29
	<hr/>	<hr/>
	100.00	100.00

The components of the subject of the second analysis, as obtained in the three operations, were—

Chloride of potassium	25·67
Chloride of sodium	8·57
Potash	5·43
Soda	12·49
Lime	6·25
Magnesia	7·03
Peroxide of iron	2·09
Phosphoric acid	15·28
Sulphuric acid	0·84
Carbonic acid	9·01
Silica	7·05
	<hr/>
	99·71

In accordance with these investigations, the white of hen's eggs, although decidedly a proteine substance, must be enumerated amongst the almost teleoxidic substances. The large quantity of silica in the white of egg, both in the teleoxidic and the anoxidic portion, is remarkable. The white of birds' eggs is equally as requisite for the formation of the feathers, which, according to recent investigations, contain a large amount of silica, as the milk of the mammalia is for the production of the bones.

Yolk of Egg.

Aqueous extract.—It exerted a strongly acid reaction upon litmus paper, and contained a considerable quantity of the earthy phosphates in solution, which were not on this occasion separated, but added to the muriatic extract. The dry mass fused into a transparent vitreous mass at a low red heat. It consisted of—

	I.	II.
Potash	10·38	9·77
Soda	5·62	7·65
Lime	11·72	11·80
Magnesia	1·45	2·04
Peroxide of iron	0·59	0·95
Phosphoric acid (undetermined)		68·74
		<hr/>
		100·95

The phosphoric acid formed metaphosphates with the bases, excepting with the peroxide of iron. In the extract itself, they did not exist in this form, but in that of acid *b*-phosphates, for it strongly reddened litmus paper; they were, however, contained in that state in the solution of the fused residue of

the evaporated mass. In accordance with the second experiment, we obtain the following arrangement for the salts:—

Monobasic phosphate of potash . .	24.57
Monobasic phosphate of soda . .	25.16
Monobasic phosphate of lime . .	41.73
Monobasic phosphate of magnesia . .	9.08
Perphosphate of iron	1.79
	<hr/>
	102.33

The calculated amount of phosphoric acid is 70.18 per cent.; experiment gave 68.74 per cent.

Muriatic extract.—The following is the composition of that of the second experiment:—

Lime	22.32
Magnesia	2.98
Peroxide of iron	3.71
Phosphoric acid	70.97
	<hr/>
	99.98

These salts are also metaphosphates, the iron compound probably being excepted. They correspond to—

Monobasic phosphate of lime . .	78.94
Monobasic phosphate of magnesia .	13.30
2Fe ₂ O ₃ , 3P O ₅	8.66
	<hr/>
	100.90

The calculated amount of phosphoric acid would be 71.89 per cent.; that found was 70.97 per cent.

Residuary carbonaceous mass.—This, like the white of egg, was also burnt in oxygen gas. Hence the results obtained in the two experiments differ; in the first much less ash was found than in the second. The latter consisted of—

Potash	7.96
Soda	6.75
Lime	13.04
Magnesia	2.04
Peroxide of iron	0.99
Phosphoric acid	64.13
Silica	2.76
	<hr/>
	97.67

These are also mostly metaphosphates; only a very small quantity of the bases can be combined with pyrophosphoric acid.

The following are the results of the experiments:—

	I.	II.
Extracted by water from the carbonized yolk }	63·73	40·95
Extracted by muriatic acid .	13·73	8·05
Ash of the remaining mass .	22·54	51·00
	<hr/> 100·00	<hr/> 100·00

These results differ very considerably, yet they show that the yolk undoubtedly belongs to the meroxidic substances.

The inorganic constituents of the entire mass of the carbonized yolk in the second experiment were—

Potash	5·94
Soda	4·82
Lime	15·79
Magnesia	2·36
Peroxide of iron . . .	1·85
Silica	0·92
Phosphoric acid . . .	68·26
	<hr/> 99·94

These are metaphosphates. The yolk of egg contains more phosphoric acid than any other organic substance treated of in this memoir.

Inorganic Constituents of Yeast (from Berlin Pale Beer).
By B. W. Bull of New York.

The yeast was washed with distilled water; the washing cannot however be perfectly effected, because the pores of the filter become so readily stopped up.

Aqueous extract.—This did not affect litmus paper; during evaporation it deposited earthy phosphates, which were added to the muriatic extract. It consisted of—

Chloride of sodium	0·69	} or of {	Na Cl	0·69
Potash	45·79		2KO + PO ₅ . . .	40·18
Soda	0·29		KO + PO ₅ . . .	57·55
Phosphoric acid	52·22		2NaO + PO ₅ . . .	0·52
	<hr/> 98·99			<hr/> 98·94

Hence the aqueous extract consisted essentially of *a*- and *b*-phosphate of potash.

Muriatic extract.—It was composed of—

Potash	33.48
Soda	0.39
Lime	9.69
Magnesia	4.79
Peroxide of iron	0.52
Sulphuric acid	0.20
Phosphoric acid	50.93
	<hr/>
	100.00

Part of the phosphoric acid is combined with the bases in the form of *b*- and part in that of *a*-phosphates. It is not easy to explain why these are not extracted from the carbonized mass with the other *a*-phosphates by water. The calculated salts are—

Bibasic phosphate of potash . .	50.39	} PO_5	28.56
Monobasic phosphate of potash . .	11.40		
Monobasic phosphate of soda . .	1.28	} KO	33.23
Bibasic phosphate of lime . . .	22.04		
Bibasic phosphate of magnesia . .	13.09		
Perphosphate of iron	1.46		
Sulphate of potash	0.44		
	<hr/>		
	100.10		

Residuary carbonized mass.—This consisted of—

Potash	28.71
Soda	0.60
Lime	2.35
Magnesia	6.36
Peroxide of iron	1.16
Phosphoric acid	60.82
	<hr/>
	100.00

or

Monobasic phosphate of potash . .	72.14	{ PO_5	43.43
Monobasic phosphate of soda . . .	1.97		
Bibasic phosphate of magnesia . .	13.91	{ KO	28.71
Monobasic phosphate of magnesia .	5.18		
Phosphate of lime ($8\text{CaO}, 3\text{P O}_5$)	4.60		
Perphosphate of iron	2.20		
	<hr/>		
	100.00		

The phosphate of lime is in this case assumed as having the same composition as that precipitated from the muriatic solution by ammonia.

The entire results of the experiments were—

Extracted by water	27·24
Extracted by muriatic acid	37·70
In the ash of the residuary carbonaceous mass	35·06

The whole constituents were— 100·00

Chloride of sodium	0·19
Potash	35·16
Soda	0·42
Lime	4·47
Magnesia	4·05
Peroxide of iron	0·61
Sulphuric acid	0·08
Phosphoric acid	54·74
	99·72

These results agree very well with those obtained by Mitscherlich.

Yeast is therefore a meroxidic substance, and possesses most analogy with flesh in regard to its inorganic constituents.

XXXVI. *Notice respecting Du Bois Reymond's Discovery of the Development of Electricity by Muscular Contraction. By Prof. BUFF of Giessen*.*

THE remarkable observation made by Du Bois Reymond, that an electric current can be excited by muscular contraction, has been called in question by Messrs. Despretz and Becquerel, who did not succeed in obtaining favourable results on repeating the experiment†. Under these circumstances it may prove of interest to describe a few experiments which I have made with a better result.

The galvanometer employed was constructed by Kleiner of Berlin; it had 3000 convolutions of a copper wire one-fifth of a millimetre in thickness. The extremities of this wire were connected, according to Du Bois Reymond's directions, with strips of platina cut out of the same sheet of metal. Each strip dipped permanently into a vessel containing a saturated solution of common salt. Notwithstanding this precaution it was found impossible to obtain an absolute and permanent uniformity of the two strips. However, on immersing the fingers in the salt water, in general only a faint current, which

* From Liebig's *Annalen der Chemie* for June 1849.

† A notice of M. Du Bois Reymond's experiments appeared in the *Philosophical Magazine* for July 1849, p. 543; Messrs. Becquerel and Despretz's observations on the same subject will be found at pp. 53, 55 of the present volume.—*Ed. Phil. Mag.*

soon decreased, was developed; but it was of such extent that the needle seldom came to perfect rest. By bracing the muscles of the hand and arm only doubtful effects were obtained, precisely as was found by the French experimenters. As the needle oscillated somewhat rapidly, seven to eight seconds to one oscillation, I endeavoured to render its astatic system more perfect, and succeeded in reducing the time of vibration to thirty seconds, *i. e.* in increasing the sensitiveness of the needle nearly sixteen times.

Nevertheless the influence of the muscular contraction was scarcely rendered more perceptible. Sometimes it was more, sometimes less obscured by accidental deflections of the needle, which it becomes the less possible to control the more the magnetic directive force has decreased. Very little was therefore to be expected from continuing to perfect the astatic system, at least with the multiplier in use, the wire of which did not appear to be entirely free from iron. Du Bois Reymond obtained a higher degree of sensitiveness by means of a larger number of convolutions, which is evidently preferable in experiments of this nature.

One method of observing the phenomenon discovered by Du Bois Reymond with less sensitive instruments, is by increasing the electromotive action excited by muscular exertion. Sixteen persons who took part in this experiment held each other's moistened hands, and on all contracting simultaneously the right, or simultaneously the left arm, they formed, as it were, a circuit of increased electromotive power. The effect on the needle was now perfectly evident, and opposite according as the right or left arm was contracted; the direction of the current was always from the hand to the shoulder. It is essential that the muscular contraction should be increased, or at least continued, until the needle begins to return, and then suddenly discontinued. Although it was found impossible to produce a greater deflection than 10° to 12° , the corresponding intensity of the current was sufficient to overcome any accidental influences; nay, even to stop a movement in the opposite direction and to reverse it.

Giessen, July 13, 1849.

XXXVII. *Observations on the Voltaic Arc.*

By M. MATTEUCCI*.

I HAVE studied the calorific and luminous phænomena of the voltaic arc, and the transference of matter, with the aid of the electro-magnetic machine which is now generally

* From the *Comptes Rendus* for September 3, 1849.

Phil. Mag. S. 3. Vol. 35. No. 236. Oct. 1849.

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employed in the application of electricity for medical purposes. With this instrument, which acts for several days with a few of Bunsen's or Grove's cells, there is between a point and a slip of platina a continual series of electric sparks corresponding to the very close interruptions of the circuit. On observing the phænomenon with the naked eye, the arc of light would be thought to be continuous; but by looking upon a disc the surface of which is painted with black and bright rays, rotating with a certain velocity, it is easy to be convinced of the discontinuity of this electric light.

In all my experiments I employed two similar points of platina, or of another metal, instead of a point and the plate as extremities between which the spark should be emitted.

I first examined the temperature of the two metallic points at the moment the electric arc was produced; and for this purpose made very near to the extremities of the points a hole which was scarcely one millimetre in diameter, and into which was inserted the point of a thermo-electrical clasp of iron and copper in communication with the galvanometer. When the experiment is carefully performed, so as to have a continuous series of sparks accompanied by a constant sound, there is also a fixed deflection of the galvanometer. I have in this manner proved and measured the difference of temperature of the positive point compared with that of the negative point, the latter being always lower. The difference varies with the metals, as was to be expected; I found it greatest with iron and copper, iron and platina, and less with lead, bismuth and zinc.

I then studied the luminous phænomena of this electric arc, which in this case was of importance, as Dr. Neef of Frankfort had never observed any light except at the negative pole. According to that author, with a very weak current there is constantly and only at the negative pole an electric light, which he calls *primary*, from its being, in his opinion, independent of the presence of the matter of the poles.

I made a long investigation of the voltaic arc obtained with the electro-magnetic machine, observing it with the aid of a microscope which magnified from 40 to 60 times. The experiment, which is very beautiful and important, succeeds best on employing iron or platina points and a very feeble current. The following are the phænomena which I constantly observed:—

1. The positive extremity is distinctly seen only in the state of incandescence; globules of red molten matter roll over its surface, which separate from it, leaving cavities, and are projected on to the negative point, where they form mushroom-like

excrescences. By compelling the two iron points to remain in contact, it is very beautiful to observe the formation in the centre of a double cone of incandescent lava of a very brilliant light; and which, resting by its bases upon the two metallic points, evidently flows from the positive to the negative pole.

2. A diffuse light is perceived similar to a flame or luminous cloud, but transparent, enveloping the two points; this light varies in colour with the nature of the metals, and resembles every other electric light produced between different metallic points. Thus it is green with copper, of a dirty yellow with zinc, and violet with platina and silver.

3. This light or flame is constantly traversed by sparkling points, similar to those which are produced by hammering hot iron: these sparks are principally produced with the iron points, and are always seen to glow outside of the electric flame.

4. Lastly, some very brilliant and mobile luminous particles, and which seem always to congregate towards the extremity of the point, appear constantly and only at the negative pole. The observation of Dr. Neef is, as regards this, perfectly accurate: it is requisite only to change the direction of the current to observe this light immediately spring from one pole to the other. If there is a drop of oil between the points, the light of the negative pole is concentrated at the extremity only, just as with an exceedingly weak current. If a somewhat powerful current be employed, the phænomena described are no longer seen distinctly, and the two poles then appear to be equally luminous.

I have likewise studied the transference of matter by the spark between the two metallic extremities, and employed for this purpose a plate and a point of similar or dissimilar metals, making the plate sometimes positive and sometimes negative. I examined only the plate with the microscope after the experiment; in every case there was a transference of the positive on to the negative metal, and *vice versa*. The circular stain which is formed upon the plate is composed of the central part, where signs of fusion are apparent, and where the metal transferred from the other pole is deposited; around this central portion there is a radiating circle of a more or less dark colour, which varies with the nature of the metals. When the plate is positive, the marks of fusion are greater, and the stains of the metal transferred from the negative pole are scarcely perceptible, whilst the border, of a dark colour, is very large. The reverse occurs when the plate is negative. When a drop of gum-water or of turpentine is interposed between the point and the plate, it soon becomes charged with

a black powder consisting of finely divided metal, and the mark formed on the plate has no border.

My principal object in making these researches being the examination of the production of light at the negative pole, in order not to have recourse to the hypothesis entertained on this subject of the light being produced by one pole and the heat by the other, I made a large number of experiments with the positive and negative points of two different metals. I found that the fixed light at the negative pole is never produced without the presence of a platina point at the positive pole, and in this case the nature of the metal of the negative point is indifferent; on the contrary, if the negative point is of platina, and the positive pole is terminated by points of iron, copper, zinc or silver, the fixed light no longer exists on the negative point, or at all events the phænomenon becomes much confused. I was thus led to suppose that the phænomenon, being dependent on the nature of the metals, and produced principally with platina, was owing to the positive pole becoming more heated, and to the particles detached from this pole and transferred to the negative pole, becoming incandescent from their very small size. It is evident that with metals which readily oxidize and burn in the air, these phænomena are no longer produced in the same manner as with platina.

There still remained to ascertain the cause of this unequal heating of the substance of the two poles. The voltaic arc which I studied is produced in a circuit which is sometimes formed by the contact of the two points, and at other times imperfectly established by the transference of matter with the spark. I passed an electric current produced by a constant battery through two cylindrical rods of iron or lead, which were in contact at their bases. Each of these rods had near its base a very small hole, into which was inserted a thermo-electrical clasp communicating with the galvanometer. I was thus able to measure the temperature developed by the passage of the current in the metallic rod near the place of interruption, or, more exactly, near the ends of the two rods in contact. With this arrangement I was easily able to convince myself that the temperature developed by the passage of the current was at its maximum near the place of interruption; and that in order to cause this temperature to vary, it sufficed to alter the reciprocal pressure of the two rods. In proportion as the pressure decreased, without any perceptible alteration in the electric current, the temperature of the rods constantly rose. Thus the thermo-electric current developed by the clasp, being from ten to fifteen degrees when the two rods were pressed hard one against the other, rose from sixty

to seventy degrees when this pressure was diminished. In like manner the disengagement of heat varies considerably when the surface of the bases of the rods in contact is either oxidized, polished, or covered with a very thin layer of graphite, oxide of iron, &c. The heat developed is always increased by the oxidation or coating of the surface of the rods with graphite powder. In this case the hottest extremity is always that communicating with the positive pole; and the most favourable case for obtaining the greatest difference of temperature between the positive and negative pole, is that in which the surface of the negative extremity being coated with oxide or graphite powder, that of the positive extremity has remained bright. Connecting this fact with that previously mentioned concerning the unequal heating of the two poles, it becomes evident, that since by the transference of matter from the positive to the negative pole the surface of the two poles experiences a different alteration, and that the greatest change takes place at the negative pole, the difference of temperature must be due, at least in part, to the difference of alteration of the surface, which is a consequence of the fact of the transference. I hope to have thus shown, experimentally, the connexion which exists between the phænomena of the voltaic arc; and I am led to consider them as depending on the transference of matter from the positive to the negative pole. On the other hand, we know from the experiments of MM. Poiret and Becquerel, that the transfer of matter from the positive to the negative pole is a phænomenon independent of the development of heat by the electric current.

XXXVIII. *Notices respecting New Books.*

A Treatise on Land Surveying. By JOHN AINSLIE. *A new and enlarged Edition, embracing Railway, Military, Marine and Geodetical Surveying.* By WILLIAM GALBRAITH, M.A., F.R.A.S.

MR. AINSLIE'S book was in the main of a practical character, but it was the work of one who knew his business well. His directions are full and precise, and his examples numerous and instructive.

Mr. Galbraith, the editor of the present edition (who is well known for his scientific acquirements and his skill in the use of instruments of the highest class), has not only thoroughly revised Mr. Ainslie's work, but he has added greatly to its value by the composition of an elaborate article on Trigonometrical Surveying and Leveling, which extends to upwards of 200 pages.

The formulæ are given in the most modern form, and their use

exemplified in surveys of considerable extent and difficulty, in which the editor has been engaged either professionally or as an amateur.

An excellent account is given of the construction and use of the chief instruments employed in astronomy and geodetic surveying, and a collection of valuable geodetic tables concludes the work.

We recommend this valuable work to all persons who take an interest in the subject on which it treats, as the only one in our language from which all necessary information may be obtained.

XXXIX. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxxiv. p. 543.]

June 8, **T**HE Astronomer Royal resigned the Chair to the Rev. 1849. R. Sheepshanks, and then delivered to the Society an oral statement, illustrated by models, "On Instruments adapted to the Measure of small Meridional Zenith Distances."

The Astronomer Royal remarked that there were three distinct ranges of observation, for which three distinct classes of instruments had been employed. The first and most comprehensive included the observations for zenith distances throughout the whole extent of the meridian, from the north horizon to the south horizon; to which observations the mural circle and similar instruments were adapted. It was evident that constructions adapted to secure this extent of observations in all their generality could scarcely be expected to avail themselves of the advantages peculiar to observations confined to one part of the heavens; and accordingly, in nearly every instance in which observations on a more limited arc of the meridian were sufficient, instruments of the other classes had been employed. The second class of instruments might be included under the name of zenith sector. With this instrument, by limiting the range of observation to six or seven degrees on each side of the zenith, a part of the arc of meridian was embraced which sufficed for most geodetical purposes; that part, both from the smaller proportion of obscuration by clouds and from the smaller uncertainty attending the calculation of refraction, was better in an astronomical point of view than the rest; the instrument also might, with great portability, be made of large dimensions, with long telescope and long plumb-line, two advantages which were highly valued in the last and in the beginning of the present centuries. Generally, its use had been confined to geodesy; although it must not be forgotten that one of the most celebrated specimens of it, namely, Bradley's zenith sector, was constructed solely for astronomical observations at a fixed observatory; still, however, that instrument had in practice been confined to the observation of a single star passing the meridian within two minutes of the zenith, and it might thus be considered as included in the third class. The third class included those instruments specially adapted to the observation of stars pass-

ing within still smaller distances of the zenith. It is evident that these, besides possessing in the highest excellence the astronomical advantages ascribed to the second class, might be expected to enjoy extraordinary freedom from the distortions produced by the weight of the instrument, and might also give great facility for measures of zenith distance by the simple use of the micrometer or the spirit-level. This was the class of instruments which the Astronomer Royal proposed as the subject of the present statement.

The Astronomer Royal announced that his discussion would be confined to a critical description of three instruments: the Greenwich 25-foot zenith tube, now dismantled; Struve's prime vertical instrument, now in use; and a construction of a reflex zenith telescope, proposed by himself, but not yet actually constructed. The second of these instruments, it is true, may be used for zenith distances of several degrees, but its peculiar advantages are connected with its use for very small zenith distances. These three instruments would be found to embody, as essential parts of their construction, the three different methods of referring to the zenith in use in modern times; namely, the plumb-line, the spirit-level, and the reflexion from the surface of quicksilver.

I. The Greenwich zenith tube was planned and constructed by Troughton, several years before Mr. Pond's direction of the Royal Observatory ceased; although, from some delays in regard to the small but essential parts, it was not brought into use till within two years of that time. It consisted of a telescope 25 feet long, revolving in azimuth on a pin at the bottom and between guides near the top, but absolutely confined to the vertical direction. Its range of observation was therefore necessarily limited to that angle in which the injurious effect of obliquity of the pencil upon the image of the star is insensible; and practically it was confined to the single star γ Draconis, at two minutes only from the zenith. The aperture of the object-glass was 5 inches; the diameter of the tube at the top 6 inches, which increased at each successive step downwards (the tube being made in five separate lengths), till it was 10 inches in diameter at the bottom. About 6 inches below the object-glass, on the outside of the tube, was a reel of silver wire; the wire passed upwards over a wheel admitting of a certain degree of end-motion, and then passed through a hole in the side of the tube, and rested between the threads of a screw, within the tube and 4 inches from its top, whose axis was horizontal (a convenient arrangement for small movements of the point of suspension), and then descended vertically. Within the great tube was a small vertical tube, nearly an inch in diameter; the top of this tube was about 10 inches below the object-glass, and here the small tube was in contact with the side of the great tube; the tube extended to the lower end, at which part it was separated about 2 inches from the side of the great tube. The plumb-line descended through this vertical tube, and passed through a hole in the bottom plate of the telescope, and there it carried the plumb-bob in a cup of water supported by hooks upon the lower plate. Just above the top of the small tube,

or 9 inches below the top of the great tube, and consequently 5 inches below the point of free suspension of the wire, a micrometer-microscope was fixed externally to the tube, on the same side on which the wire tube is placed, for reading the position of the wire. Very close to the bottom of the tube, but on the side opposite to that on which the wire tube is placed, another micrometer-microscope was placed for reading the position of the lower part of the wire; the wire tube here was perforated to permit vision of the wire. This microscope was almost completely buried in the great tube (its micrometer and eyepiece alone projecting from it); it was not, however, firmly screwed down; it was thrust in by hand into a hole (where it was held by friction only) in a sliding-plate within the great tube, which sliding-plate was moved by external screws. It is evident that, if these microscopes were firm, and if the wire retained in all positions of the instrument its peculiarities of curvature, &c. in constant relation to the *instrument*, while its general direction of dependence had respect to nothing but the *direction of gravity*, the readings of the micrometer-microscope would give most accurate information upon the position of the great tube with respect to the vertical (the observation of the sides of the plumb-wire being exceedingly delicate). Below the lower plate of the great tube was the eyepiece of the telescope, a 4-glass diagonal eyepiece, its eye-end projecting horizontally below the eyepiece of the lower micrometer-microscope, and nearly to the same distance. With this was viewed the wire (carried by a micrometer, to which was given for distinction the name of grand micrometer), by which the bisection of the star was effected. The lower plate of the telescope was supported by four arched pieces of brass, attached to the lower plate at its circumference, and united about 6 inches below its centre; and at the place of union was the pin which turned in the foot-plate. The intervals between the brass arches left room for the eyepiece, the micrometer, and the plumb-line cup. The foot-plates had adjusting screws, which were, however, very rarely used. The upper guides of the great tube were about 5 feet below the top; they were carried by an enormous iron tube, which inclosed the great tube to within about 4 feet of the bottom, and was there supported by large iron arches rising from four large iron pillars, which at some distance surrounded the pier of the foot-plates, being placed at the angles of a square, and leaving a clear space of $4\frac{1}{2}$ feet between each pillar and that opposite to it.

When the present Astronomer Royal took charge of the Royal Observatory, he found the grand micrometer of this instrument in the following state:—The wire-plate of the micrometer carried also the long heavy eyepiece projecting sideways more than 6 inches; the long micrometer-screw extended nearly from side to side of the lower plate of the tube, was supported at its extremities by projections from that lower plate, and was tapped through the case of the diagonal mirror, thus acting at a distance of about 1 inch from the wire-plate. The supports of the ends of the screw, upon whose firmness the correctness of the action of the screw must entirely

depend, were thin vertical arches attached to the bottom plate only by small thumb-screws, and these arches sustained the force of the micrometer-screw sideways. Three arrangements more entirely opposed to the just principles of construction of micrometers can scarcely be conceived. The wire-plate ought not to be strained sideways by a projecting weight, and ought not to carry any weight which will increase the friction of its movement; the action of the screw ought to be in the plane of motion of the wire-plate; and the supporting point, or *appui*, of the screw ought to be perfectly firm.

The following changes were therefore made without delay. The long screw was laid aside, and in its place was used a micrometer-screw or stalk attached to the end of the wire-plate in the usual way; and the micrometer was made in every respect like an ordinary micrometer, the micrometer-head carrying the concave-screw which embraces the stalk and draws it towards the micrometer-head, the action of the screw being in the plane of the wire-plate and directed longitudinally across the middle of the wire, and the drawing action of the screw being resisted by springs at the opposite end of the wire-plate. The motion of the micrometer was limited in this construction to about one-tenth of the whole breadth of the end-plate of the telescope; but this was far more than was required to measure the zenith distance of γ Draconis; it was, however, made subservient to the measure of nearly the whole breadth in the following way:—On the fixed plate of the micrometer ten crosses of wires were fastened, as nearly as possible at equal distances, and on the moveable plate were fixed eleven wires at nearly the same distances. By means of the crosses on the fixed plate and the micrometer-movement, the interval from each wire on the moveable plate to the next wire was ascertained, in terms of the micrometer-screw, with great accuracy, and therefore the aggregate or distance of extreme wires was very well known. The intervals were also known in seconds of arc, by observations of the transits of stars when the instrument was turned to a position distant 90° in azimuth from its usual position. Thus the value of the screw, and the elements for making an observation in any part of the whole range available, were completely obtained. With this construction of micrometer, the *appui* of the micrometer-head was almost close to the bottom plate, and was perfectly firm. The eyepiece was carried by another sliding-plate, moving in grooves unconnected with the micrometer; and it was moved by a separate rack and pinion. It is presumed that in this form the grand micrometer was perfectly trustworthy.

In the use of this instrument it is evidently necessary to observe the same star successively in reversed positions of the instrument, the micrometer-head being on the north side in one observation and on the south side in the other observation; and the values of the grand-micrometer reading must be corrected in each observation by quantities depending on the two micrometer-readings of the plumb-line, in order to obtain for the two observations the angular distances of the star from definite (though imaginary) lines in space, equally

inclined to the vertical, one on the north side, the other on the south side; and if this double operation be effected at one transit of the star, the result for the star's zenith distance is obtained without any computation of the star's corrections, and without any reliance on the permanence of the state of the instrument for more than a few minutes; but if the observation with micrometer-head north is made at the transit of one day, and that with micrometer-head south at the transit of another day, it is necessary to compute the change in the star's place (which can be done with undoubted accuracy), and also to be assured that no change has taken place in the relative position of the various parts of the instrument. For the details of the calculations of every kind applicable to these cases, it is best to refer to the introductions to the volumes of Greenwich Observations from 1837.

For a considerable time, the instrument, not being furnished with two wires on the micrometer-plate at a distance nearly equal to double the star's zenith distance, could not be used for the double observation at a single transit (as the short duration in the field of view did not leave time enough for the numerous turns of the micrometer, with change of the observer's position, &c.), and it was therefore reversed after the completion of each day's observation. The results of the observations were not satisfactory, and the Astronomer Royal determined on so fitting up the instrument that it might be used for the double observation at a single transit.

The arrangements obviously necessary were, to fix on the micrometer-plate two wires at distances nearly equal to double the star's zenith distance, and to determine their interval accurately; and to provide stops for the reversion of the instrument, for the movement of the eyepiece, and for the movement of a part of the illuminating apparatus, which it was necessary to shift at each observation. But another change, the necessity for which seems to have been generally overlooked, then suggested itself. It is unsafe to consider any wire as absolutely straight: when it has once received a bend, even no more than the bend of passing round a reel, it retains a portion of that bend although stretched even to the breaking-point; and some of those curved parts of the wire may be under the microscopes by which the position of the plumb-line is checked. Now this is unimportant, provided that the bend is always turned in the same direction relatively to the instrument; for then its only effect is to alter by a constant quantity the correction to the value of the grand-micrometer reading, which constant disappears on taking the difference of the two corrected grand-micrometer readings, upon which, in fact, the determined zenith distance depends. But if the position of the bend changes relatively to the instrument, the result is affected with error. Now it is extremely probable that the position of the bend will change; that is, that the plumb-line will turn relatively to the instrument at every reversion, but more particularly so at rapid reversion; and some method must be adopted to prevent this turning.

For this purpose the Astronomer Royal adopted the following

construction:—On the side opposite to the eyepiece, and attached to the bottom plate of the telescope, a wooden box was carried out horizontally, its bottom being nearly as low as the foot-plate, and its top at the level of the bottom plate of the tube; and upon the end of this box was planted a wooden plumb-wire tube, connected at its top with a frame attached to a higher part of the great tube (upon which frame the wooden plumb-wire tube and the horizontal box were, in fact, suspended). From the top of this wooden tube was suspended (with screw-adjustment for moving it to or from the principal plumb-line) a second plumb-line, 40 inches long, and distant from the principal plumb-line about 12 inches. The two plumb-lines supported in the horizontal box the two ends of a bar 12 inches long, and to this bar were attached one or two plumb-bobs. A little consideration of the theory of parallel forces will show that, if the distance between the plumb-lines at the top and the bottom is the same (the criterion of which is, the distinctness of the principal plumb-line in the field of view of the lower microscope, whether the second plumb-line be used or not), the relative movements of the principal plumb-line for varying inclinations of the grand tube are the same as if the plumb-bob were immediately attached to it. At the same time, the turning of the wire is effectually prevented.

The instrument was used with this construction to the spring of 1848, when it was finally dismantled. The results of observation were more accordant than they were before introducing the last modification, but they were not superior to those derived from the mural circle. There can be no doubt that the remaining errors were greater than could be attributed to mere imperfection of observation, and that they must originate in some fault of the instrument. Generally speaking, the greatest errors coincided with the greatest irregularities in the readings of the lower plumb-line microscope. It has already been pointed out that the fixation of that microscope was not very firm, and the irregularities may have originated in this weakness. Or the great tube may have twisted sensibly in the reversion. Or the plumb-line may have been bound by spider-threads, from which it was to a certain degree set free by the movement of reversion (for it was found almost impossible to keep the wire-plate free from spider-threads, the uniform temperature and the constantly vertical position of the telescope being probably comfortable to those animals). Whatever the distinct cause might be, the Astronomer Royal considered that the instrument had failed, and that its failure was owing to the dependence on the plumb-line; and he expressed his hope that he might never again be compelled to use an instrument relying for its verification upon a plumb-line.

II. The applicability of a transit instrument in the prime vertical to the determination of latitude of place from assumed polar distance of the star, or *vice versa* (the polar distance of the star being greater than the co-latitude of the place), has long been known; but it seems to have attracted more particular attention since it was used by Struve in the determination of the difference of latitudes at the ex-

tremities of his arc of meridian parallel to the Baltic. Whatever be the construction of the transit instrument used, the proper method of observation is the following:—The error of level of the axis being ascertained, the instrument is directed to the star, while it is yet north of the eastern prime vertical, and the transit of the star is observed over each of the wires preceding the middle of the field; the position of the instrument being continually changed, so that the oblique transit is observed over the centre of each wire. When the star has passed the wire next before the middle, the instrument is reversed; and the passage of the star, now on the south side of the eastern prime vertical, is observed over the same wires as before, but in the opposite order. The error of level of the axis is then ascertained. Then, when the star is approaching the western prime vertical from the south, the instrument being still in its second position, the error of level of the axis is again ascertained; then the transit of the star is observed again over the same wires; before it passes the middle of the field, the instrument is reversed to its first position; then the transit of the star, now on the north side of the west prime vertical, is observed again over the same wires. Finally, the error of level of the axis is ascertained in this position.

The reduction of the observations is made in the following form, each wire being treated separately. The projection of each wire on the sky is a small circle whose pole is in the north point N of the horizon; and if α be the angular distance of that wire from the line of collimation, $90^\circ - \alpha$ will be the radius of the small circle when the star is seen on it north of the prime vertical, and $90^\circ + \alpha$ when the star is south of the prime vertical. Form the spherical triangle NPS; let A be the hour-angle from the meridian, or the supplement of the angle at P; l the latitude of place; δ the star's north polar distance. Then when the star is north of the prime vertical,

$$\cos(90^\circ - \alpha) = \cos l \cdot \cos \delta - \sin l \cdot \sin \delta \cdot \cos A_1;$$

and when the star is south of the prime vertical,

$$\cos(90^\circ + \alpha) = \cos l \cdot \cos \delta - \sin l \cdot \sin \delta \cdot \cos A_2.$$

Adding these two equations, we obtain

$$\cot l \cdot \cot \delta = \cos \frac{A_1 + A_2}{2} \cdot \cos \frac{A_1 - A_2}{2}.$$

A_1 is half the interval between the first transit east and the second transit west, and A_2 is half the interval between the second transit east and the first transit west, in both cases converted into arc. Thus by the observations on each wire we determine with great facility l from δ , or δ from l , without any knowledge of the distance of that wire from the line of collimation.

The mean error of level of the instrument may be applied to l before forming the logarithm of $\cot l$ (supposing that it is the object of the observation to determine δ). Or a correction may be applied for it (which will be different for different stars) to the result obtained with a constant value of l . The latter is the course followed by M. Struve in reducing the observations of which I shall shortly speak.

The advantages peculiar to this observation are, that it is not affected by ordinary refraction; and that its scale, being one of time, is exact to a degree which is unapproachable in other ways. Bad, indeed, must the clock be of whose rate we are not certain within 1^s per diem; therefore the uncertainty on our time-scale cannot practically amount to $\frac{1}{80000}$ part of the whole, and that on the results of the observation, as depends on this cause, cannot amount to $\frac{1}{40000}$ part of the whole. But the Astronomer Royal stated, as the result of his own experience, that an accuracy of $\frac{1}{1000}$ in the determination of a micrometer-scale is almost more than can be hoped for. Other advantages which it possesses are common to other reversible instruments. Thus if there be a constant optical fault in the image of a star, produced by defects in the object-glass, that fault will produce opposite effects in the first and second positions of the instrument: if the pivots be mis-shapen, as if there be a hump upon one, yet if the form of the two Y's is similar, that hump will take the same bearing upon the east side of one Y in one observation as upon the west side of the other Y in another observation, and its effects will be annihilated in the result: if the pivots are unequal, the effects of the inequality are similarly annihilated. But to secure all these advantages, the two following instrumental points must be secured in the construction: the instrument must admit of having the level applied to it while the telescope is in the position of observation; and it must admit of being reversed with ease and rapidity. To the obtaining of these objects, the construction devised by M. Struve, and carried out by Messrs. Repsold, is particularly well adapted. This instrument is supported upon Y's carried by two stone pillars, about 6 feet high and 46 inches apart, from outside to outside; the *outside* face of the pillars being vertical, and the *inside* faces inclined to the vertical. The axis of the instrument has its bearings upon the two Y's; but the telescope (7 feet long) attached to this axis is *on the outside of one of the pillars*; a counterpoise at the other end of the axis being on the outside of the other pillar. Between the two pillars is the reversing apparatus, which also carries the ordinary counterpoises. It consists of a vertical shaft, sliding through holes in cross-bars which are fixed to the piers, and prevented by a fillet upon it from turning until it is raised to a certain height; this vertical shaft carries a T head about 33 inches long, at the extremities of which are the lifting-forks, and also the fulcra of the ordinary counterpoises. The counterpoises act by means of levers to support a bar about 41 inches long, at the extremities of which are the friction-rollers, which at all times support the principal part of the weight of the instrument. The vertical shaft does, therefore, at all times support the fulcra pressures of the counterpoises; and when the instrument is raised for reversion, by bringing up the vertical shaft so that the lifting-forks at the ends of the T head come in contact with the axis of the instrument, the only additional load which is put upon the vertical shaft is that pressure which was left as residual weight upon the Y's on the stone pillars, a pressure which, in the practice of the German astronomers,

is very small. Two massive lever-counterpoises are therefore provided below, which act upwards under the foot of the shaft: and if these are adjusted to support the shaft when the instrument is not on the lifting-forks, they will also practically nearly support it when the instrument is on the lifting-forks: so that a very trifling effort of the hand is then necessary to raise the shaft with the instrument. This small effort is given through a winch, acting by means of bevel-gearing upon a large circular nut, which works on a screw-thread cut upon the shaft, and bears vertically upon one of the cross-bars: and thus by an exertion which appears almost fancifully small, the instrument is raised for reversion, is turned round (the telescope being placed horizontally), and is deposited in its new position. Of this reversing apparatus the Astronomer Royal spoke with great praise. Not only is the reversion effected with a rapidity and ease scarcely to be conceived, but also the counterpoises are acting in the same way, and the general strains upon the instrument are almost exactly the same as when it is in ordinary use. The Astronomer Royal has borrowed this principle (although he has applied it in a different form), in the apparatus which he has adopted for raising the proposed transit-circle for the Royal Observatory, in order to give opportunity for the adjustment of its collimators.

The general form of the axis, being unencumbered by any telescope crossing it, is evidently well adapted to the application of the level at all times; a thing which is always important, but particularly necessary for the German instruments, which are frequently counterpoised almost to the last ounce, so that there is not in them the same security for the bearing of the pivots in their proper position as in our instruments, in which a far greater residual weight is left.

The instrument, therefore, and its auxiliary apparatus, are most admirably adapted to securing the two advantages, of easy reversion and application of the level in the position of observation, which are so desirable for this instrument. But these advantages, in the opinion of the Astronomer Royal, are very dearly bought by an entire abandonment of that mechanical firmness of connexion between the telescope and the axis which is obviously necessary to make the observations trustworthy. The kind of firmness which is required is that which retains the telescope in a position at right angles to its axis; the same, in fact, as that required for a transit-instrument. We laugh at the transit-instruments of the last century, in which, while great pains were taken to secure length of axis between the bearings, the central connexion was left very weak; and we praise the modern transit-instruments, in which the central connexion has been made successively larger and larger, and not least so by the German artists; and of which a more admirable specimen cannot be cited than the Edinburgh transit-instrument, made by Messrs. Repsold, the constructors of Struve's prime-vertical instrument. Yet in this prime-vertical instrument, that important connexion is probably very far weaker than in any transit-instrument that ever was made. The whole support of the 7-feet telescope,

upon the firmness of which support the value of the observations entirely depends, is a single perforated pivot, 4 inches in diameter, on one side of the telescope. It is true that the telescope is sustained, as regards the strain of its own weight upon the small pivot, by an internal concealed counterpoise (for no one who has mastered all the external counterpoises of a German instrument is therefore to suppose that he has possessed himself of all the applications of that principle in the interior of the instrument), whose fulcrum is in the perforated pivot, near the telescope, and whose weight is within the hollow case at the other end of the axis, which appears to the eye like a large counterpoise connected with the external axis. But this counterpoise, while it delivers the pivot from the ordinary strains to which it would be exposed from the weight of the telescope, does in no degree diminish the effect of what, perhaps, are really more formidable, the accidental strains produced by pressures on the ends of the telescope, or other accidental forces, or irregularities of forces, not taken into account in the construction of the instrument. For instance, if the Y's were slightly irregular, so that the principal bearing of the pivots on the northern Y was an inch nearer to the face of the pier than that on the southern Y (a thing which it would be nearly impossible to discover by examination), the difference in the bend of the axis in the two positions of the instrument would probably be so considerable that every result would be worthless.

In the opinion of the Astronomer Royal, the asserted consistency of results hitherto obtained with this instrument proves nothing. Although discordance proves the existence of some fault, accordance does not negative the existence of very great faults. The Astronomer Royal cited the expression of Bouguer, who, after much painful experience in the construction of zenith sectors, in different forms, for the measure of the Peruvian arc, came at last to the conclusion that no agreement of results proved their truth, unless the logical correctness of construction of the instrument gave reason *à priori* for believing that the results would be good.

One defect to which this instrument is liable was pointed out by M. Struve himself to the Astronomer Royal. It cannot be assumed that the temperatures of the external faces of the two piers are the same; and if they be not, the effects of their radiation upon the telescope-tube must be different*.

The Astronomer Royal then remarked, that though perhaps the form of the ordinary transit-instrument would not give in their full extent the same facilities, yet the great importance of securing the admirable firmness and excellent connexions of the transit-instrument made it desirable for us to attempt to unite with them, as far as possible, the peculiar conveniences of Struve's instrument. In

* The prime vertical instrument of M. Struve, constructed by Repsold, is fully figured in plates 32, 33 of M. Struve's magnificent work, *Description de l'Observatoire de Poulkova*, Saint Pétersbourg, 1845. Drawings of the Greenwich zenith tube, and models of Struve's prime vertical instrument and of the reflex zenith tube, may be seen at the apartments of the Royal Astronomical Society.

regard to the reversion, by a forked apparatus rising from the floor, there is no difficulty; the only difficulty is in the application of the level while the instrument is in the position of observation. There appears to be no valid reason prohibiting the use of one of the following constructions. The level-frame might consist of two bars extending from the pivot-forks towards the telescope, there interrupted in their straight course, and united by a large oval ring, through which the telescope could play; each bar must then carry a short spirit-level. Or it might consist of a parallelogrammic frame, strengthened in the middle by a ring through which the telescope could play, the two short sides being attached to the pivot-forks, and the two long sides carrying two long levels. But a different form of instrument may be suggested, allowing of the application of the ordinary single level at all times, and apparently embodying the conveniences of Struve's form, while it does not abandon the strength of the usual form. The transit-instrument may be made in the form of the letter T (the horizontal line of the T representing the axis of rotation), the object-glass of the telescope scarcely rising above the thick part of the axis. As the weight of the telescope is entirely on one side of the axis, it must be balanced by counterpoises carried by a very large fork; the stalk of the fork being within the telescope-tube, the two arms of the fork being in the axis of the transit, and resting within the pivots for a fulcrum, but projecting out beyond the pivots; and the two prongs of the fork projecting towards the object viewed by the telescope, and being loaded with the counterpoises at their ends.

If, however, we rely upon our transit taking the same bearing in the Y's after reversion, a very much simpler principle may be used, dispensing entirely with the level. It is only necessary, after having made the observations on the north side of the east prime vertical (as already described), to reverse the instrument and to observe on the south side of the east prime vertical by reflexion in a trough of quicksilver; then to reverse and observe on the south side of the west prime vertical by reflexion; then to reverse again, and to observe, by direct vision, on the north side of the west prime vertical.

The Astronomer Royal expressed himself confident that in some of these ways the advantages of Struve's construction might be secured, with the additional guarantee for the goodness of the results, that they are obtained with an instrument of firm mechanical construction.

The Astronomer Royal then explained that his attention had been directed to these constructions by the necessity created by the present condition of astronomy for a few accurate observations, at whatever trouble obtained, of stars near the zenith. Struve's instrument is now employed on three stars only, and M. Struve is satisfied if of each of these stars he can obtain eight observations in a year. At Greenwich there is special need of observations of one star, namely γ Draconis, a star that may with propriety be considered as the birth-star of English Astronomy. Unfortunately no instrument on the prime vertical principle is applicable to this star, because it passes

north of the zenith of Greenwich. The Astronomer Royal therefore has been compelled to endeavour to devise an instrument which shall be firm in its connexions, and shall also be applicable to the observations of stars on both sides of the zenith. The following is the construction which he proposes for this purpose.

III. The proposed reflex zenith telescope is founded upon these considerations. If an object-glass be placed with its axis vertical; and if a pencil of light fall on it from a star near the zenith, and pass from the object-glass with its axis still inclined to the vertical, but with the rays of the pencil in a state of convergence; and if a trough of quicksilver be placed below it at a distance somewhat less than half the focal length of the object-glass; the pencil of light will then be reflected from the quicksilver with its axis still inclined in the same degree to the vertical, and with the rays still in the same state of convergence, and will again pass through the object-glass, and will form an image of the star at a very short distance above the object-glass, and at a distance from the axis of the object-glass depending on nothing but the star's zenith distance and the focal length of the object-glass. Although we cannot fix on the axis of the object-glass, yet we know that if the object-glass is turned through 180° , the image will now be formed at an equal distance from the axis of the object-glass, but in an opposite direction relatively to the frame of the object-glass; and therefore the distance between the two positions of the image, as measured by a micrometer attached to the frame of the object-glass, will be double the distance of either image from the axis of the object-glass, and will therefore be a measure of the star's zenith distance. The peculiar advantage of this construction is, that it requires no firmness of connexion except that of the micrometer with the frame of the object-glass. The mercury-trough may be totally unconnected with the rest of the instrument. The firmness of support of the object-glass is unimportant; for, however much the object-glass is pushed sideways (giving the same movement to the image of the star), the micrometer is equally pushed sideways, and the measure of the image is not disturbed. The peculiar disadvantage is, that the light must be reflected from quicksilver, and must pass again through the object-glass, and must be transmitted through a four-glass eyepiece with a diagonal reflector, so that the whole loss of light will be considerable. But this disadvantage appears insignificant in comparison with the advantage.

Theoretically, the place of the image will be affected by a local fault in that part of the object-glass through which the rays pass the second time. But, practically, the existence of such a fault is unlikely; and its effect, if any existed, would be proportional to the distance of the image from the object-glass, and therefore small: and moreover it could be ascertained and measured by previous experiment on the object-glass. No injury, therefore, to the results, and no real inconvenience to the observations, would arise from such a fault.

The only risk to which this construction appears to be exposed is
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the following. If the object-glass with the micrometer attached be tilted, the place of the image of the star upon the micrometer will be disturbed by a small quantity, unless the plane of the micrometer be at one certain distance from the object-glass. It is therefore an important matter to determine what that certain distance will be. It is easily seen that if the plane of the micrometer pass through one of the points called *focal centres*, this condition is satisfied. For a ray from a vertical star passing through the focal centre (the object-glass being inclined), will be refracted in a parallel vertical direction to the quicksilver, and will then be reflected back from the quicksilver in the same line, and will by refraction be made to pass again through the same focal centre; and, supposing the distance of the quicksilver to be properly adjusted, so that the image of the star is formed on the micrometer, that image will be at the focal centre whatever be the inclination of the object-glass. The place of the focal centre may be determined by an apparatus, in which the object-glass is planted in a frame that admits of being slid in a direction perpendicular to its plane, the sliding-cell being upon a board which turns in its own plane on a pin; a beam of light is directed upon the lens through two narrow slits; and a telescope is placed on the opposite side of the object-glass to receive the light; when, by trial of sliding the frame, a position is so determined that, upon rotating the turning-board, through a large angle, the position of the beam of light as seen in the telescope does not change, it is then certain that the focal centre is in the axis of rotation. If the micrometer can be conveniently fixed at this distance from the object-glass, the accidental inclination of the object-glass will be unimportant; if the micrometer is at any other distance, there will be a very small correction to the measures, depending on the inclination of the object-glass; and it will be proper that a small spirit-level be attached to the object-glass frame for the measure of the inclination. Perhaps in any case this addition will be prudent.

The adjustment to focal length will depend upon nothing but the distance between the object-glass and the quicksilver; and a power of altering this distance must be retained. It is proposed to do this by moving the tube, in which the object-glass turns, up or down by a rack-and-pinion motion, the tube and its load being as nearly as possible balanced by a lever-counterpoise. It is also proposed that a smaller quicksilver-trough, communicating with the larger, should carry a float, from which a light stalk should rise by the side of the object-glass frame; if this stalk be made of the same material as the micrometer, a scale upon the stalk will indicate the value of the micrometer-scale as corrected for thermal expansion, and as affected by any change of focal length.

Although such an instrument may be adapted to the observation of any stars which pass within a field of view expressed by the breadth of the object-glass, yet some conveniences of fixation are gained by limiting it to the one star γ Draconis. The following are the details of mounting proposed by the Astronomer Royal. The micrometer necessarily requires two metallic bars crossing the object-

glass; and upon reversing the object-glass, with micrometer attached, the two bars will again occupy the same position in space. Consequently there will be no additional interruption of light if the support of the prism-reflector of the eyepiece be two bars in the vertical planes which pass through the micrometer-bars, carried by crooked projections from the tube in which the object-glass turns (and therefore not reversed with the object-glass). And as the prism-reflector intercepts a small portion of the object-glass excentrically, a corresponding portion equally excentric on the opposite side must be intercepted by a small plate carried by the two bars, in order that the diffraction-disturbance of the star's image may be symmetrical. One lens of the eyepiece will be below the prism-reflector, and one close to its vertical, or nearly vertical, face (unless it be thought preferable to produce the effect of lenses, by grinding the faces of the prism-reflector to spherical forms): the remaining lenses of the eyepiece (namely, the field-glass and the eye-glass) will be fixed in a tube, entirely exterior to the object-glass and therefore causing no additional loss of light, carried by a crooked projection from the tube in which the object-glass turns. These crooked projections permit the micrometer-heads and reversing-handle, &c. to pass, in the reversion of the object-glass.

Notwithstanding the great simplicity and compactness of the essential parts of this instrument, the Astronomer Royal thinks it desirable that it be so arranged that a double observation can be made at each transit of the star. It is necessary for this purpose that two wires be fixed in the micrometer plate, at an interval corresponding nearly to the double zenith distance of the star. By fixing temporarily in the immediate field of view of the eyepiece a cross of wires, or by planting a microscope for the occasion above the micrometer frame, the interval between these two wires may be found very accurately in terms of the revolutions of the micrometer; and by fixing other wires on the micrometer plate at intervals as nearly as possible equal to that interval, and by using a series of microscopes fixed for the occasion, the micrometer-scale intervals between all these wires may be very accurately found. And by turning the object-glass and attached micrometer to a position 90° distant from either of the positions of observation, and observing the transits of zenithal stars over all the wires, the intervals in arc may be found. The combination of these will give the best possible information on the value of the micrometer-scale, and on the intervals of the wires.

A simple micrometer might be used for the observation; it would, however, have these disadvantages; that the micrometer must be read between the two observations, and that the observer could not use the same hand in the two actions upon the micrometer head. The Astronomer Royal proposes a more complex micrometer, in which the micrometer B, to which the bisection-wires are attached, is carried by and has its screw-*appui* in a micrometer A; and the micrometer A has its screw-*appui* in the cell of the object-glass. There is no difficulty in so arranging this that the movement of micrometer

A shall not tend to disturb the relative place of micrometer B, and that the movement of micrometer B shall not tend to disturb the absolute place of micrometer A (A and B standing in exactly the same relation as the tangent-screw of a mural circle, and the micrometer in its telescope): and then the observation may be conducted in this manner. One wire being very nearly in the position for bisecting the star, A will be read, and the bisection will be completed by B. Without waiting to read the micrometer head, the object-glass, &c. will be reversed, and the second bisection will be completed by A. Then A and B will be read. It will be proper sometimes to effect the reversion in the opposite order: and for this purpose, using the same hand on the micrometer, B must be read before beginning, and the first bisection must be completed by A, and after reversion the second bisection must be completed by B. In either case the complete double observation may be obtained with great rapidity, but without the smallest hurry.

The Astronomer Royal expressed his belief that an instrument thus constructed might be expected usually to give results accurate to one-tenth of a second of arc.

The Astronomer Royal then stated that, before attaching any name to this construction, he had requested the assistance of the Master of Trinity College, Cambridge, whose authority in a philological question of this kind is undisputed. Doctor Whewell has fixed on the name "The Reflex Zenith Telescope;" a name which appears to express with singular accuracy the peculiarities of its construction, and which the Astronomer Royal hoped would be universally adopted.

XL. Intelligence and Miscellaneous Articles.

ON CARBONATE OF LIME AS AN INGREDIENT OF SEA-WATER.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

September 6, 1849.

IN the current Number of the Philosophical Magazine appears an abstract of a paper by Dr. Davy On Carbonate of Lime as an Ingredient of Sea-water, from which, according to the author's inferences, it appears that "carbonate of lime, except in very minute proportion, does not belong to water of the ocean at any great distance from land." But with all deference I would submit that this may be true as regards the surface only; and that the bottom of the sea, even at its greatest distance from land, may be equivalent to "proximity to coasts," the point urged by Dr. Davy.

That there are grounds for such a supposition, and for believing that no inconsiderable quantity of carbonate of lime does exist at the bottom of the ocean far from land, is apparent in Darwin On Coral Reefs, where instances are given of living corals and corallines being taken up from great depths; and taking the presumption that the base of some of the remote coral islets of the Pacific, whose perpen-

dicular wall descends to immense depths, is the same as the summit, we have further evidence that carbonate of lime does exist, or has existed, in such situations—far from coasts.

Again, Sir James C. Ross, in his *Voyage to the Southern Seas* (vol. i. pp. 202, 203, 207, 208), relates that on one occasion the dredge was put over in 270 fathoms and brought up living coral; a day or two afterwards corallines were obtained in 300 fathoms; and speaking of the maintenance of organic life under pressure, he observes, “hitherto we have not been able to determine this point beyond a thousand fathoms, but from that depth several shell-fish have been brought up with the mud.” Sir James appears to believe that their existence at greater depths is not impossible; for he pursues, “as we know they can bear the pressure of one thousand fathoms, why may they not of two?”

I leave it to your judgement whether the foregoing remarks are worthy of publication, and remain,

Your obedient Servant,

WALTER WHITE.

GOLD IN CERTAIN MINES OF THE DEPARTMENT OF THE RHONE.

MM. Allain and Bartenbach state that the copper mines of Chessy and of Saint Bel (Rhône) have been the objects of interesting experiments; the result of which is, not only that the copper and zinc which the pyrites contains may be easily extracted, but that it contains also at least 1-10,000 of gold. According to analyses, the numerical results of which are not stated by the authors, the pyrites contains sulphur, iron, zinc (about 8 per cent.), copper (about 5 per cent.), silica, arsenic, and gold, 1-10,000 at least. This discovery of gold has naturally led to the performance of a series of operations, in order to find an economical method of extracting this metal. Although the experiments are not entirely finished, the authors consider that the separation of this small quantity of gold is easy and economical, and that the copper, zinc, and sulphuric acid obtained, will partly cover the expenses of extraction; the method is briefly as follows:—The sulphur and arsenic being expelled by roasting, and the oxides of zinc and copper formed dissolved by sulphuric acid, the residue, which is composed of silica, sesquioxide of iron and gold, is to be washed, and then treated with a cold aqueous solution of chlorine; after some hours' action a solution of chloride of gold is obtained, from which the metal is reduced by the usual processes; the chlorine in this case does not act upon the sesquioxide of iron.—*L'Institut*, Août 8, 1849.

ON THE ANALYSIS OF PLANTS BY INCINERATION.

BY M. CAILLAT.

The author, who is professor at the Agricultural Institute of Grignon, is of opinion that incineration, generally hitherto employed

for obtaining the inorganic matters of plants, yields incorrect results; that the sulphates contained in the plant so treated are in great measure decomposed, and that the sulphuric acid or sulphur escapes in large proportion among the gaseous products of combustion.

It occurred to M. Caillat to treat the residues of plants, such as lucern, trefoil and sainfoin, with dilute nitric acid, and he succeeded in separating almost the whole of the mineral substances which they contained; so that the pulpy residue of 10 grammes of the substance employed, after washing and drying, burnt readily, leaving only 18, 20, or 22 milligrammes of ashes. This small residue consisted of silica and a little peroxide of iron, substances both insoluble in the acid employed. This method of treating plants always yielded the author a larger proportion of mineral substances than he obtained from the same quantity of the same plants by incineration; and in certain vegetables he found a much greater quantity of sulphuric acid than has hitherto been stated.

M. Caillat states, that he found by experiment that the loss of sulphuric acid occasioned by incineration is derived from the decomposition of a part of the sulphate of lime. Thus on intimately mixing with starch and water a known quantity of pure and calcined sulphate of lime, and incinerating the mass, the collected ashes did not contain as much sulphuric acid as the sulphate of lime employed.

The author has also stated another direct experiment, which shows that the sulphate of lime converted into sulphuret of calcium by the influence of the organic matter, at a high temperature, is partly converted into carbonate of lime by the action of the oxygen of the air. The oxygen gas, burning at once the sulphur of the sulphuret and a portion of carbon interposed, forms sulphurous acid, which is evolved, carbonic acid, part of which remains combined with the lime, facilitating thereby the displacement of the sulphur.—*L'Institut*, Août 8, 1849.

BLUE ARSENIATE OF COPPER. BY M. REBOULLEAU.

The author had proposed to employ the above-named compound both as an oil and as a water colour; but he has since found that, owing to the action of the oil on the oxide of copper, the colour becomes bluish-green; in fact, that the arseniate acts with oil like verditer and other blue preparations of copper.

If equal parts of common arseniate of copper and neutral arseniate of potash be mixed and heated, the compound melts, and yields on cooling a fused, perfectly transparent mass of a bluish-green colour, a vitreous fracture and very fusible. The resulting compound is a double arseniate of potash and copper obtained in the dry way by M. Berthier's process. Whilst the double arseniate is in perfect fusion, if one-fifth of its quantity of powdered nitrate of potash be projected into it, brisk effervescence ensues, and a large quantity of nitric oxide is disengaged. The crucible then removed from the fire contains a magnificent blue substance, formed of sub-arseniate

of potash and arseniate of copper, in the state of a double salt. When this compound reduced to powder is treated with water, the double salt is decomposed; the water carries off the arseniate and nitrate of potash, and leaves a precipitate of arseniate of copper of an admirable blue colour.—*L'Institut*, Août 1, 1849.

ON METHYLAMINE AND ETHYLAMINE.

M. Adolphe Wurtz has described the preparation and properties of the above compounds, belonging to a class of substances to which he has given the name of compound ammonias (*ammoniaques composées*).

Methylamine.—The process by which this base was obtained does not differ from that employed by chemists in preparing ammonia. Perfectly dry hydrochlorate of methylamine is mixed with twice its weight of quicklime, and the mixture is introduced into a long tube closed at one end, so as to occupy half of it. The other half is filled with fragments of potash, to which a tube is adapted for conveying the gas to an air-jar filled with mercury. The tube is to be slightly heated, beginning at the closed end; methylammoniacal gas, displaced by the lime, is abundantly liberated, and received in the air-jar filled with mercury.

Methylamine thus prepared is a non-permanent gas. At about 32° F. it condenses into a very moveable liquid. Its odour is strongly ammoniacal. Its density was found to be 1.13; it is therefore rather more dense than atmospheric air. The experimental result is rather higher than the theoretical, which is 1.075. This is undoubtedly owing to the temperature at which the experiment was made, the gas being too near its point of liquefaction.

Methylammonia gas is the most soluble of all gases hitherto known. At the temperature of about 53° F. one volume of water dissolves 1040 volumes; a higher temperature diminishes this solubility, as might be expected. At 77° F. water dissolves only 959 times its volume.

Like ammonia, it is instantaneously absorbed by charcoal, and also resembles it in immediately rendering reddened litmus paper blue; when exposed to the vapour of hydrochloric acid, it forms a very thick white smoke. Like ammonia, it absorbs an equal volume of hydrochloric acid gas, and half its volume of carbonic acid gas. It is distinguished from ammoniacal gas by the circumstance that, when exposed to flame, it takes fire and burns with a yellowish flame.

The composition of methylammonia gas is represented by $C^2 H^5 N = 4$ vols.

An elegant and rapid analysis of this gas is effected by heating it with potassium in a bent tube: cyanide of potassium is formed and hydrogen is evolved, $C^2 H^5 N + K = C^2 NK + H^5$.

The solution of methylamine has the strong odour of the gas itself. Its taste is caustic and burning. Iodine reacts upon the solution of methylamine, and is converted into a powder of a garnet-red colour;

and the liquor, which hardly becomes coloured, contains hydriodate of methylamine IH , $\text{C}^2\text{H}^5\text{N}$. The red insoluble compound formed as described is analogous to iodide of nitrogen.

The salts of magnesia, alumina, manganese, iron, bismuth, chromium, uranium, tin, lead and mercury, are precipitated by methylamine as they are by ammonia.

The salts of zinc are at first precipitated, and of a white colour; but the precipitate redissolves in a large excess of the reagent; the salts of copper are precipitated of a bluish-white colour; excess redissolves the precipitate, forming a solution of a deep blue colour; the salts of cadmium, nickel and cobalt, are precipitated by the solution of methylamine, but excess does not redissolve the precipitates.

Nitrate of silver is completely precipitated by methylamine; the oxide of silver dissolves readily in an excess of the reagent. When this solution is suffered to evaporate spontaneously, a black substance is precipitated which is analogous to fulminating silver. This substance does not explode either by a blow, or by the action of heat; chloride of silver itself dissolves in solution of methylamine.

Chloride of gold is precipitated of a brownish-yellow; an excess readily dissolves the precipitate, forming an orange-coloured solution. A concentrated solution of chloride of platina gives with methylamine a crystalline deposit in orange-coloured scales, consisting of a double hydrochlorate of methylamine and platina.

Ethylamine.—This base was obtained by decomposing hydrochlorate of ethylamine by lime, in the same manner as methylamine. Excepting that as ethylamine condenses readily, and is liquid at common temperatures, the delivery-tube is surrounded with ice, or still better by a freezing mixture; the ethylamine distilled at a moderate heat condenses in the receiver.

In a pure state, it is a light, very moveable, and perfectly limpid liquid. It boils at about 64°F . When poured on the hand it volatilizes, instantly producing very considerable cold. It gives out an extremely penetrating ammoniacal odour; its causticity may be compared to that of potash. Ethylamine renders litmus paper which has been reddened strongly blue; when exposed to hydrochloric acid it yields dense white vapours. Every drop of acid added to it produces a whistling at the moment of its mixing with the base. Barytes and potash remain in contact with it at common temperatures without alteration. When exposed to flame, ethylamine takes fire and burns with a bluish flame. It mixes with water in all proportions, giving out much heat, and yielding a solution the basic properties of which are exactly similar to those of methylamine; hydrate of copper is, however, most readily soluble in ethylamine.

Chloride of platina is not precipitated by ethylamine. When a solution of ethylamine is mixed with oxalic æther, the mixture soon becomes turbid; alcohol is formed; very slender crystals separate of a compound which is to oxamide what ethylamine is to ammonia; it is ethyloxamide, the composition of which is represented by $\text{C}^6\text{H}^6\text{N}^2\text{O}^2$. The composition of anhydrous ethylamine is represented by the formula $\text{C}^4\text{H}^7\text{N}$.

One hundred parts give—

	Experiment.		Theory.
Carbon	54.4	..	54.3
Hydrogen	15.9	..	15.5
Nitrogen.....	30.9	31.3	31.2

Comptes Rendus, Août 13, 1849.

ON VALERAMINE OR VALERIC AMMONIA.

M. A. Wurtz states that cyanate of amylene, which he obtained by distilling sulphamylate of potash with cyanate of potash, is easily decomposed by solution of potash. The reaction, favoured by the action of heat, gives rise to carbonate of potash and a volatile base, valeramine, which distils when the solution of potash is boiled. It is found in the receiver in solution in water which is volatilized and condensed with it.

On saturating this solution of valeramine with hydrochloric acid, a perfectly neutral hydrochlorate is obtained, which by evaporation is obtained in the form of white scales, greasy to the touch, very soluble in water and soluble in alcohol; it does not deliquesce in the air. By analysis it yielded—

	Experiment.	Calculation.
Carbon	48.2	48.5
Hydrogen.....	11.4	11.3
Chlorine	28.3	28.7
Nitrogen	11.5

This analysis agrees perfectly with the formula $\text{HCl}, \text{C}^{10} \text{H}^{13} \text{N}$.

The accuracy of this formula was verified by analysing the double salt which is precipitated when concentrated solutions of chloride of platina and hydrochlorate of valeramine are mixed. As it is considerably soluble in water, it is advantageous to add a little alcohol to the mixture. After having collected and pressed the precipitate, it is to be redissolved in boiling water; on cooling, it precipitates in fine scales of a golden-yellow colour, the composition of which is represented by the formula $\text{HCl}, \text{C}^{10} \text{H}^{13} \text{N}, \text{Pt Cl}^2$, as deduced from the following results of the analyses:—

	Experiment.	Calculation.
Platina	32.6	32.9
Chlorine	36.0	36.5
Carbon	20.4	20.5
Hydrogen.....		
Nitrogen	4.8	4.8

When hydrochlorate of valeramine is distilled with lime, the volatile valeramine passes over into the receiver. When pure, this base is liquid; its taste is both burning and bitter, and its odour strongly ammoniacal. It is soluble in water, and the solution precipitates the

salts of copper, an excess redissolving the precipitate, yielding an azure blue solution; this effect is, however, produced with greater difficulty than with ethylamine, methylamine or ammonia. Valeramine also precipitates nitrate of silver; the precipitate is of a fawn-colour, and adheres like a resinous mass to the bottom of the vessel. An excess of the reagent whitens and completely dissolves it. Chloride of silver is dissolved by valeramine, but with more difficulty than by ammonia.—*Comptes Rendus*, Août 13, 1849.

ON CHLOROFORM. BY MM. SOUBEIRAN AND MIALHE.

Two liquids are sold in commerce under the name of chloroform; and though of very different origin, they are considered as identical, and are accordingly substituted for each other. Nevertheless there are notable differences in their properties: one, derived from the reaction of hypochlorite of lime upon alcohol, possesses all the properties which one of the authors of this memoir has assigned to chloroform; it is that which may be called normal chloroform; the other, obtained by the action of hypochlorite of lime on pyroxylic spirit, is so different from the first, that the authors have submitted them to a minute examination in order to discover the cause of the difference.

The chloroform from pyroxylic spirit, which the authors provisionally call methylic chloroform, although possessing the same physical appearances as normal chloroform, has quite a different odour: this odour is not sweet and agreeable, but empyreumatic and nauseous. Its density is less than that of common chloroform; the latter being 1.496, the former 1.413*. Its boiling-point appears also to be not so high; lastly, the inhaling of methylic chloroform is so far from being easy and agreeable, that it occasions general uneasiness, followed by heaviness of the head, continued nausea, and sometimes vomiting.

Such differences in the properties of these two liquids induced the authors to think that they did not possess the same composition, or that the properties of one of them was masked by some foreign substance.

On the first hypothesis, it might be thought that chloroform, which does not belong to the same chemical type as alcohol, and which is formed by the powerful reaction of chloride of lime, may be a different body according as it is formed from alcohol, which belongs to the ethyle series, or from pyroxylic spirit, which belongs to that of methyle. It may happen, also, that the difference de-

* The authors remark in passing, that the density of chloroform, stated by M. Liebig to be 1.480, is too light; they constantly obtained it of 1.496 at 54° F. The difference is unquestionably derived from the presence of some foreign body, which it was not known how to separate from the chloroform, as the authors will presently show.

pend on the greater or less condensation of the methyle, which may be supposed to exist in both chloroforms.

In this uncertainty the authors undertook analyses, the results of which will be hereafter stated. The second hypothesis supposes the identity of the chloroforms formed from alcohol or from pyroxylic spirit. The difference in this case would be derived from the presence of a foreign body. This opinion is the best founded. In fact, the authors found, by endeavouring more perfectly to rectify methylic chloroform on chloride of calcium, that the salt which remained in the retort contained a quantity of a peculiar oil, easily separable by washing with water: by repeated rectifications, 30 grammes of this oil were obtained from 500 grammes of some commercial chloroforms.

This new substance was liquid, and of an oily consistence. At first yellowish, it became colourless by simple rectification. It had a very peculiar and strong empyreumatic odour, which was recognized as the cause of the peculiar odour of methylic chloroform; it was lighter than water; submitted to distillation in a retort containing a thermometer, it began to distil at 185° F.; but this temperature was so far from remaining fixed, that it rose to 244° F. At this moment the operation was interrupted, because the thermometer, on account of the small quantity of the remaining oil, did not dip into it.

This increase of temperature during the distillation evidently denotes a mixture of various compounds. This heterogeneous oil burnt readily with an intense sooty flame. The presence of chlorine among the products of combustion showed that this body was a constituent part of its elements.

The chloroform, several times rectified for the production of this oil, did not retain the slightest pyrogenous odour which characterizes it. Some chemical reaction was then searched for, which, while it would not react upon the chloroform, was capable of separating or destroying the oil that it contains. After some trials, concentrated sulphuric acid appeared to answer the purpose: it occasioned the impure chloroform to assume a brownish-red colour, the intensity of which was greater in proportion to the quantity of oil in the mixture. When colour ceased to be produced in the chloroform, it no longer retained the empyreumatic odour.

The authors state that, without the fear of incurring any sensible error, they were able to analyse the chloroform thus purified, and to examine and compare its properties with those of normal chloroform. The analyses, densities, in the liquid state and that of vapour, were all found to be similar; and satisfactory proof was obtained that only one chloroform exists; and that which is prepared from pyroxylic spirit does not differ from that obtained from alcohol, when all the oil above described has been separated. It must, however, be admitted, that the complete separation of this oil was not effected; there still remained a very minute portion of it, so small as not to affect the density or the results of analyses; but it was distinguishable by its odour, which remained after the evaporation of a considerable quantity of chloroform. It was peculiarly sensible in the

operation of determining the density of its vapour; the matras after the operation retained the peculiar odour of this chlorinated methylic oil. It is almost impossible to get rid of the last traces of it: it resists the action of concentrated sulphuric acid, even when the chloroform is long exposed to it.

There do not, then, exist two chloroforms; the cause of the apparent difference is the presence of the peculiar oil produced by the reaction of chloride of lime on pyroxylic spirit.

This fact being established, search was instituted to determine whether, during the preparation of chloroform from alcohol, an analogous substance was formed. Experiment confirmed suspicion; the rough chloroform was washed first with water and afterwards with carbonate of soda; it was long kept in contact with chloride of calcium to combine with the water; lastly, it was filtered and distilled in a water-bath from a glass retort. There remained in the retort a liquid aromatic substance, but of an odour very different from that of chloroform; the proportion was very small, for not more than 40 grammes were obtained from 20 kilogrammes of chloroform.

This oil differs essentially from that obtained from chloroform prepared from pyroxylic spirit. It is more dense than water, has a peculiar acrid and penetrating odour, not at all resembling that of the other oil; but like it, it was found to be a mixture of different compounds; for the thermometer, which on heating was 134° F. at the commencement of ebullition, became as high as 242° F.; and the temperature would undoubtedly have been increased if the experiment could have been tried with a larger quantity. All these compounds are chlorinated, as proved by examining the results of their combustion.

It results from the preceding statements, that as chloroform obtained from pyroxylic spirit cannot be entirely deprived of its pyrogenous odour, it ought not to be employed for inhaling. The presence of chlorinated oil, in the small quantity even in which it exists in chloroform obtained with alcohol, has a strongly-marked influence in its employment: it is to it that must be attributed the uneasiness, nausea and vomiting, occasioned by inhaling chloroform.

It follows that it is absolutely necessary to rectify chloroform by distillation in order to separate the foreign body which it contains, and moreover the distillation should be stopped sufficiently soon.

In concluding, the authors remark that chloroform, like hydrocyanic acid, when poured on filtering paper, partly evaporates so rapidly as to occasion sufficient cold to solidify the remainder in white silky tufts, which remain for a few seconds.

The authors conclude from the facts above stated, that—

1st. Chloroform prepared from pyroxylic spirit is identical with chloroform properly so called.

2nd. The purification of methylic chloroform is too difficult to admit of its advantageous substitution for normal chloroform.

3rd. During the preparation of chloroform there is always pro-

duced a certain quantity of pyrogenous, essential chlorinated oil, the action of which on the animal œconomy is extremely hurtful.

4th. It is indispensable to free chloroform from this chlorinated essential oil, by not continuing the rectification too long.—*Journ. de Pharm. et de Ch.*, Juillet 1849.

ON THE PREPARATION OF NITROGEN GAS.

BY M. B. CORENWINDER.

The preparation of nitrogen is attended with several inconveniences: there is no one of all the processes employed by which it can be obtained rapidly and pure, without much precaution and the use of complicated apparatus. This circumstance induced the author to publish the method by which he obtains in a few minutes a large quantity of this gas, and in a state of absolute purity, as shown by experiments related below.

This process is derived from the decomposition of nitrite of ammonia, which, as is well known, is resolved by heat into nitrogen and water; but as this salt is difficult to prepare, it is replaced by a mixture of alkaline nitrite of potash and hydrochlorate of ammonia, a mixture which contains the elements of nitrite of ammonia and chloride of potassium.

In order to obtain the nitrite of potash in a proper state, it is requisite to employ a solution of caustic potash, of density 1.38, and to pass into it nitric oxide obtained from the decomposition of one part of starch by ten parts of nitric acid, until a distinctly acid product is obtained, and afterwards to add to it caustic potash, so as to render it decidedly alkaline.

The nitrite thus prepared may be kept without undergoing alteration, so that a quantity of it may be prepared; and when nitrogen is required, it is sufficient to mix concentrated solutions of one volume with three volumes of hydrochlorate of ammonia, and to heat the mixture in a retort by a charcoal fire; the disengagement soon commences, and continues with perfect regularity.

As it is necessary, in order to have the gas pure, that the nitrite should be alkaline, it will be expected that a small quantity of ammonia will be evolved; but this disengagement is unattended with any inconvenience; if the nitrogen gas be required completely deprived of this alkali, it is sufficient to pass it through water acidulated with sulphuric acid contained in a bottle.

The following experiments left no doubt as to the purity of the nitrogen gas thus obtained:—

1st. After having deprived it of ammonia in the manner above stated, it was passed into a bottle containing a mixture of zinc, sulphuric acid and water, and consequently in the presence of nascent hydrogen.

The experiment was continued for a considerable time; and when

it was terminated, there was not the slightest indication of the presence of ammonia in the solution. The result was equally negative with sulphuret of iron and dilute sulphuric acid.

2nd. A certain weight of copper recently reduced by hydrogen was placed in an organic analysis glass tube, and subjected for about half an hour to the action of a red heat and a current of washed nitrogen, which had been afterwards dried by pumice-stone and sulphuric acid, taking the precaution of not heating the tube till after all the atmospheric air had been driven out by the disengagement of the gas. The experiment was repeated several times, and no alteration was observable in the exterior appearance of the copper, nor had it increased in weight.—*Ann. de Ch. et de Ph.*, Juillet 1849.

ON THE QUANTITY OF AMMONIA CONTAINED IN ATMOSPHERIC AIR. BY M. R. FRESENIUS.

In the following table the author has arranged the results of his own experiments and those of MM. Grager and Kemp; 1,000,000 parts of air contain—

	Ammonia.	Oxide of ammonium.	Carbonate of ammonia.
Grager	0.333	0.508	0.938
Kemp	3.880	5.610	10.370
Fresenius { day	0.098	0.153	0.283
{ night . . .	0.169	0.257	0.474
Fresenius' mean	0.133	0.205	0.379

The proportional quantities of ammonia found by these chemists are—

Fresenius	1 day
Fresenius	1.7 night
Grager	3.4
Kemp	37.5

M. Fresenius observes that he is far from setting a high value upon his experiments, though he took great care in all his operations; he is, however, of opinion that he may draw the following conclusions from them :—

1st. The determinations hitherto arrived at respecting the quantity of ammonia contained in the air have given too large results; for the differences between the analyses are so great, that it is impossible to attribute them to a change in the composition of the air, unless the change has been accidentally produced and purely local.

2nd. To determine with exactitude the quantity of ammonia contained in the atmosphere, much larger quantities than have hitherto been acted upon must be submitted to experiment. It would be

proper to operate upon at least 12,000 to 15,000 litres in order to collect at least 10 milligrammes of platina.

3rd. The author states that, until researches have been made on a large scale, his experiments may serve as approximative.

If it be true that night air is richer in ammonia than the air of the day, this fact may be explained by the phænomena presented by the nutrition of plants, by the circumstance that the ammonia which accumulates in the air during the day and during the night is dissolved and precipitated by the dew at sun-rise.—*Ibid.* Juin 1849.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1849.

Chiswick.—August 1. Very fine: clear. 2. Very fine: slight rain. 3. Slight rain: overcast: cold at night. 4. Clear and fine. 5. Fine: cloudy. 6. Cloudless: very fine. 7. Fine: overcast: rain. 8. Clear: very fine: lightning. 9. Foggy: very fine: heavy showers. 10. Hazy: very fine: clear. 11. Clear: cloudy: rain. 12. Overcast: clear: rain. 13. Showery. 14. Cloudy and fine. 15. Very fine. 16. Showery: very clear at night. 17—19. Fine. 20. Cloudy. 21—23. Very fine. 24. Very fine: hazy. 25. Uniformly overcast: very fine. 26, 27. Very fine. 28. Very fine: slight rain at night. 29. Overcast. 30. Dry haze: rain at night. 31. Hazy: cloudy and fine.

Mean temperature of the month	62°·91
Mean temperature of Aug. 1848	58 ·74
Mean temperature of Aug. for the last twenty-three years	62 ·18
Average amount of rain in August	2·41 inches.

Boston.—Aug. 1. Fine. 2. Fine: rain P.M. 3. Cloudy. 4. Fine. 5—7. Cloudy. 8. Cloudy: rain early A.M.: thermometer 79° 3 P.M. 9. Fine: rain P.M. 10, 11. Cloudy: rain P.M. 12. Fine. 13. Cloudy: rain P.M. 14. Cloudy. 15. Fine. 16. Cloudy. 17. Fine. 18. Cloudy: rain A.M. 19. Fine. 20—23. Cloudy. 24—26. Fine. 27—29. Cloudy. 30. Cloudy: rain P.M. 31. Fine.

Applegarth Manse, Dumfries-shire.—Aug. 1. Fair. 2, 3. Fair: a few drops P.M. 4. Fair and warm. 5, 6. Fair: warm: cloudy P.M. 7. Frequent showers. 8. Fair and fine: beautiful day. 9. Very warm: thunder: showers. 10. Heavy shower: very warm. 11. Very heavy rain: thunder. 12. Rain: river flooded. 13. Heavy rain. 14. Showers A.M.: cloudy P.M. 15. One shower: dull and cloudy. 16. Heavy showers: hail. 17. Wet A.M.: fine: thunder P.M. 18. Frequent showers. 19. Fair: calm: cloudy. 20. Fair and fine. 21. Fair, but dull and cloudy. 22. Light drizzling showers. 23. Showers frequent, not heavy. 24. Fair, but cloudy. 25. Shower during night: cleared. 26. Showers A.M.: fine. 27. Fair and bracing: harvest day. 28. Fair, but dull: rain P.M. 29. Fair and fine all day. 30. Rain throughout. 31. Fair and fine: dull: cleared.

Mean temperature of the month	56°·7
Mean temperature of Aug. 1848	53 ·7
Mean temperature of Aug. for the last twenty-five years ...	57 ·1
Mean rain in Aug. for twenty years.....	9·60 inches.

Sandwich Manse, Orkney.—Aug. 1. Drizzle. 2—4. Drizzle: showers. 5. Fog: cloudy. 6. Fog. 7. Cloudy. 8. Rain: fog. 9. Hazy: fog. 10. Hazy: cloudy. 11. Cloudy: rain. 12. Clear: clear, aurora. 13. Bright: clear. 14. Drizzle: rain. 15. Clear. 16. Bright: cloudy. 17. Showers. 18. Bright: cloudy A.M. 19. Damp: cloudy. 20. Drizzle: damp. 21. Rain: fine. 22. Rain: clear. 23. Clear. 24. Clear: cloudy. 25. Rain. 26. Bright: showers. 27. Bright: cloudy. 28. Cloudy: showers. 29. Drizzle: damp. 30. Bright: damp. 31. Clear: fine.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Appleearth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

Days of Month.	Barometer.						Thermometer.						Wind.				Rain.			
	Chiswick.		8 1/2 a.m.	Dumfries-shire.		Orkney, Sandwick.	Chiswick.		8 1/2 a.m.	Max.	Min.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
	Max.	Min.		Max.	Min.															
	9 a.m.	9 p.m.	9 1/2 a.m.	9 1/2 p.m.	9 a.m.	9 p.m.	9 a.m.	9 p.m.	9 a.m.	9 p.m.	9 a.m.	9 p.m.	9 a.m.	9 p.m.	9 a.m.	9 p.m.	9 a.m.	9 p.m.	9 a.m.	9 p.m.
1849. Aug.	30.117	30.053	29.53	29.90	30.02	29.95	30.08	30.08	64	49	50 1/2	49	sw.	nw.	nw.	nw.
1.	30.139	30.091	29.63	30.01	29.95	30.08	30.08	30.08	60	60	46 1/2	50	sw.	nw.	nw.	nw.
2.	29.987	29.965	29.50	29.98	29.97	30.06	30.08	30.06	59	59	50	49	ne.	n.	n.	n.
3.	29.979	29.900	29.52	29.95	29.88	30.01	29.96	29.96	53	66 1/2	44 1/2	49	ne.	nw.	ne.nw.	n.
4.	29.939	29.863	29.46	29.87	29.89	29.93	29.95	29.95	51	54	52 1/2	50	se.	se.	w.	n.
5.	30.048	30.041	29.55	29.96	30.00	30.01	30.07	30.07	61	59	53 1/2	51 1/2	w.	wnw.	s.	e.
6.	30.062	29.960	29.57	29.94	29.82	30.07	29.97	29.97	64	64	50	52	w.	se.	ese.	e.
7.	29.889	29.838	29.47	29.78	29.75	29.86	29.84	29.84	68	68	50	54	sw.	se.	s.	se.
8.	29.752	29.681	29.16	29.68	29.60	29.78	29.75	29.75	85	70	57	58	sw.	calm	e-s.	calm
9.	29.873	29.737	29.16	29.64	29.70	29.74	29.82	29.82	69	69	56	56 1/2	sw.	calm	sse.	e.
10.	29.906	29.861	29.30	29.64	29.55	29.82	29.70	29.70	67	66 1/2	54 1/2	52 1/2	s.	s.	se.	e.
11.	29.764	29.679	29.14	29.45	29.30	29.53	29.42	29.42	73	56	67	55	sw.	ssw.	sw.	e.
12.	29.624	29.570	29.00	29.19	29.20	29.24	29.14	29.14	64	59 1/2	56 1/2	58	sw.	w.	sw.	w.
13.	29.763	29.674	29.12	29.34	29.47	29.14	29.29	29.29	61	60	53	57	sw.	w.	sw.	w.
14.	29.910	29.865	29.39	29.55	29.55	29.41	29.50	29.50	63	59 1/2	56	52	sw.	w.	sw.	w.
15.	29.793	29.763	29.20	29.48	29.54	29.58	29.56	29.56	62	56	49 1/2	54	sw.	s.	sw.	n.
16.	29.945	29.872	29.38	29.68	29.86	29.83	29.92	29.92	60	61	43 1/2	48	w.	w.	sw.	n.
17.	30.016	29.993	29.53	29.90	29.95	29.90	30.02	30.02	60	60	46	51	w.	w.	sw.	n.
18.	30.272	30.145	29.73	30.03	30.10	29.99	29.99	29.99	59	61	42	50 1/2	n.	calm	ne	n.
19.	30.337	30.325	29.81	30.13	30.15	29.94	30.06	30.06	63	55	53 1/2	55	w.	calm	w.	sw.
20.	30.332	30.230	29.80	30.15	30.10	30.03	30.02	30.02	65	66	55	59	sw.	calm	sw.	sw.
21.	30.183	30.126	29.63	30.00	30.00	29.99	29.88	29.88	67	69	53	59	sw.	calm	sw.	sw.
22.	30.132	30.125	29.62	30.08	30.08	29.98	30.06	30.06	64	63	55	59	sw.	n.	sw.	sw.
23.	30.132	30.115	29.67	30.10	30.02	30.13	30.00	30.00	66	66	58	57	sw.	calm	sw.	sw.
24.	30.190	30.097	29.57	29.93	29.94	29.82	29.75	29.75	63	63	53	55	ne.	calm	sw.	sw.
25.	30.100	29.985	29.44	29.83	29.85	29.66	29.56	29.56	64	64	56 1/2	55	sw.	sw.	sw.	w.
26.	29.983	29.939	29.45	29.78	29.80	29.63	29.78	29.78	66	51	60	50	sw.	sw.	sw.	n.
27.	29.975	29.927	29.45	29.79	29.71	29.78	29.71	29.71	68	58	59	48	sw.	sw.	s.	sw.
28.	29.896	29.883	29.45	29.79	29.81	29.74	29.81	29.81	72	65	68	56	sw.	n.	sw.	n.
29.	29.858	29.822	29.45	29.76	29.68	29.73	29.74	29.74	76	60	68	55	n.	n.	sw.	sw.
30.	29.864	29.844	29.30	29.73	29.78	29.80	29.89	29.89	75	53	66	53	n.	n.	w.	sw.
31.	29.993	29.934	29.45	29.807	29.807	29.811	29.819	29.819	63	63	51	52
Mean.	29.993	29.934	29.45	29.807	29.807	29.811	29.819	29.819	63.2	63.0	51.2	54.61
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XLI. *On the Notation of Crystals.*

By EDWARD J. CHAPMAN, Esq.*

IT may be fairly assumed that an efficient system of crystallographic notation should comprise the following essential points,—simplicity, brevity, and capability of being verbally expressed. It should be sufficiently simple as to be retained and applied with the slightest possible effort of the memory, and sufficiently brief as to admit of being placed upon the usual drawings of crystals. The symbols, moreover, of which it is composed, should be so constituted as to be readily convertible into ordinary language.

There can be but one opinion as to the necessity of the first of these conditions, even if the utility of the two latter be questioned. It is, however, very desirable, especially for those who are studying the science, that the second condition should also be fulfilled; for, by the arbitrary and irregular lettering of crystals, much trouble and chance of misconception is obviously occasioned to the student—sometimes one, and sometimes another letter, being employed to designate the same kind of plane; and that, frequently, within the compass of a few pages. The importance, furthermore, of the third condition is so great, that the little attention which seems hitherto to have been paid to it is much to be wondered at. In addressing a class, amongst numerous other instances, it is absolutely essential that the notation should be a verbal one.

Of all the systems at present in use, that of Lévy and Dufrénoy can alone be said to embody the above desiderata; but its merit in this respect is, on the other hand, marred by the following objections, which form a serious drawback to its adoption. In the first place, totally different planes in the same crystallographic group are frequently denoted by a

* Communicated by the Author.

common symbol. In the Monometric or regular system, for instance, the sign a^1 expresses both the planes of the octahedron and the cube; and the "primary form," often an arbitrary one, must be stated before we can determine to which solid the symbol is intended to apply. The sign b^1 , again, in the same system, may denote either the planes of the rhombic dodecahedron, or those of a particular leucitoid, or even of the cube. The only method of avoiding this imperfection is by the arbitrary assumption of one primary form for the entire group; but this, which is literally done by the German school, is so manifest a contortion of nature to convenience, that the French mineralogists have wisely rejected its employment. Another disadvantage in the system of Lévy and Dufrénoy, is the non-recognition of the important bearings which the different crystallographic axes have on the study of the science; at least, these axes play no part in their notation.

In this particular the German systems possess an undoubted superiority; but the employment of arbitrary and often hypothetical primary or fundamental forms renders them objectionable. Their practical application, moreover, is by no means free from difficulty and inconvenience; for even the abbreviated system of Naumann is not sufficiently brief to allow of the symbols being placed upon the ordinary figures of crystals; neither can it be communicated verbally with any ease. That it possesses great uniformity no one can deny; but it may be questioned whether this very uniformity do not occasion an unnecessary effort to the memory, in the recognition of the designated planes. Dana, who adopts Naumann's system, has been forced, in the lettering of his crystals, to make use of another method; but this, although ingenious, is not exact enough for the purposes of notation—for which, indeed, it was never intended by its author,—nor can it be verbally employed. I need not extend these observations by referring to the works, however meritorious, of Professor Miller, Mr. Griffin, and various other authors; because in no one case are the three essential points, indicated above, fulfilled; owing either to the length, or to the peculiar construction of the notations*. For these reasons I have attempted to frame a system which shall serve both for the let-

* These remarks will, I am sure, be received in the spirit in which they are made; for, in the words of one to whose researches crystallography first owed its rank as a science, "la critique, lorsqu'elle est juste et modérée, loin d'altérer l'estime que se doivent réciproquement ceux qui courent la même carrière, ne fait que les exciter à de nouveaux efforts. Une telle critique ne déplaît jamais qu'à des esprits superficiels, ou gâtés par les éloges outrés de leurs contemporains."—*Romé de l'Isle, Cristallographie*, 8^{ve}, vol. i. p. xxiii. (second edition), 1783.

tering and for the notation of crystals, in comprising strictly the separate conditions of accuracy, simplicity, brevity, and capability of being verbally expressed. The proposed method is based entirely upon the position and relations of planes to their crystallographic axes, and has no dependence upon the *external configuration* of any fundamental form. The various planes are also denoted by a few individual letters, so chosen as to assist the memory; and not by the position of certain signs placed before, above, or around, a single letter, as in the system of Naumann. The advantages which accrue from this arrangement will, I think, be seen as we proceed.

All crystallographic planes may be included under the terms of *prismatic* and *pyramidal*; the latter comprising closed forms*, and, with the exception of the basal planes of the oblique systems, all inclined summit-planes.

PRISMATIC PLANES I designate by *consonants*; PYRAMIDAL PLANES by *vowels*†. Prismatic planes may be either *monaxial* (cutting one axis), or *diaxial*, and in one system *triaxial*. Pyramidal planes may be either *diaxial*, *triaxial*, or *tetragonal*—the latter, of course, only in the hexagonal system.

Monaxial prismatic planes may be of three principal kinds, according to the system of crystallization. In the monometric or regular system they are of one kind, and are lettered P. In the dimetric or pyramidal system, they are of two kinds; so that the letter P (from *πίναξ*, *table*, as in Naumann's *Basische Pinakoid*) is retained to designate the basal planes, or those which cut the vertical axis; and the letter M (from *μόνος*) is employed for the vertical planes. Finally, in the other systems we have P for the basal, M for the front and back vertical, and L for the lateral or side vertical planes, which cut but one axis.

Diaxial prismatic planes are of one kind in the dimetric, hexagonal, trimetric (rhombic), and monoclinic (oblique rhombic) systems; and, as such, are lettered D (from *δύς*, *two*). In the triclinic system they are of two kinds, D and B (*bis*, *two*), the respective positions of which will be shown further on. *Triaxial* prismatic planes can be only met with in the hexagonal system: they are lettered T.

The following symbols are consequently sufficient to denote all kinds of prismatic planes:—P; M; L; D; B; and T;

* The cube omitted.

† This idea is derived from Haüy's method of designating planes on primary edges by consonants, and those on angles by vowels; but it must be borne in mind that many inclined summit-planes are by this method denoted by *consonants*, for the two systems are fundamentally quite distinct.

although of D and B various series may occur, as $D\frac{1}{3}$, $D\frac{1}{2}$, D, D2, D3, &c. To this, however, we shall revert as we proceed.

Diaxial pyramidal planes are of one kind in the monometric, dimetric, and hexagonal systems, and are lettered A. In the trimetric or rhombic system they are of two kinds, denoted respectively by A and E; the former being situated in front when the crystals are in position, and the latter at the sides, or upon the *acute* angles of the rhombic prisms. A is therefore associated in position with the prismatic planes M, and E with L. In the monoclinic system the diaxial pyramidal planes are of three kinds—A, E, and I. The planes E, in sets of four, are situated at the sides; A is the *front* plane at the top of the crystal, and the *back* plane below; I is opposed to A. Finally, the triclinic system possesses four distinct sets of diaxial pyramidal planes:—A, AA, E, EE. These are, however, common to but few minerals; and they are very rarely, if ever, all present at the same time.

Triaxial pyramidal planes are of one kind in the monometric, dimetric, hexagonal, and trimetric systems, and are designated by the symbol O. In the monoclinic system they are of two kinds, O and U; the former being situated in the principal position, that is to say, in front and above. The planes U are therefore the upper planes behind, and the lower planes in front. Lastly, in the triclinic system, these planes may be of four kinds:—O, OO, U and W—but they rarely occur together in the same crystal.

The *tetragonal* planes of the hexagonal system are of one kind, and are lettered Y.

Pyramidal planes are consequently denoted by

A; E; I; AA; EE; O, U, OO, W; and Y;

of all of which various series may occur, but more especially of A, E, I, O, U, and Y; as, $\frac{1}{3}O$, $\frac{1}{4}O$, $\frac{1}{5}O$, $\frac{1}{6}O$, O, 2O, 3O, &c.

The relations and notation of these different planes are exhibited more distinctly in the following table; by which it will be perceived, that by the enunciation of a single letter, the position of the designated plane is immediately called up before us: because, by this letter, we know at once whether the plane be a prismatic or a pyramidal one; and furthermore, the number of axes which it cuts, and consequently its position.

	Prismatic Planes.	Pyramidal Planes.
Monaxial	P; M; L.	
Diaxial	D, $D\frac{1}{m}$, Dm ; B, $B\frac{1}{m}$, Bm .	A, $\frac{1}{m}A$, mA ; E, $\frac{1}{m}E$, mE ; I, $\frac{1}{m}I$, mI ; AA, EE.
Triaxial	T	O, $\frac{1}{m}O$, mO ; U, $\frac{1}{m}U$, mU ; OO; W.
Tettraxial	Y, $\frac{1}{m}Y$, mY .

It is, however, necessary, for the complete elucidation of the proposed method, to examine separately the notation appertaining to each crystallographic group. In the following scheme numbers greater than unity are represented by m or n , and those less than unity by $\frac{1}{m}$. Placed *before* a letter, these refer to the *vertical* axis; and when placed after a letter, to one of the other axes, as will be explained in the observations attached to each system.

Monometric System.

Monaxial form :—

The cube, P.

Diaxial forms :—

The rhombic dodecahedron, A.

Tettratrishexahedrons, $\frac{1}{m}A$.

Triaxial forms :—

The regular octahedron, O.

Leucitoids or icositetrahedrons, $\frac{1}{m}O$.

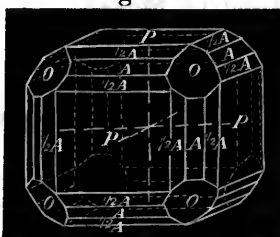
Trisoctahedrons, mO .

Hexakis-octahedrons, mOn .

Regular tetrahedron, $\frac{O}{2}$.

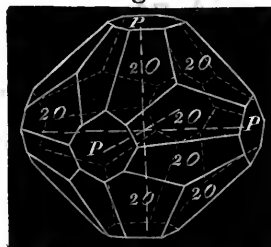
Other hemihedral forms, designated after a similar manner, $=\frac{x}{2}$.

Fig. 1.*



Copper: Siberia.

Fig. 2.



Galena: Clausthal.

Observations.—In the less simple forms of this system, that

* For several of these examples I am indebted to the beautifully executed plates of Dufrénoy's Atlas, *Traité de Mineralogie*, 1846. I have, however, added the axes to each figure, so as to render the positions of the different planes more readily apparent.

face has been chosen for the basis of the notation which cuts the horizontal axes, for the time being, at equal distances; so that, with the exception of the hexakis-octahedrons, in which a single face of this kind does not occur, these horizontal axes may always be considered *unity*. The symbol of the leucitoids becomes thus $\frac{1}{m}O$, and that of the trisectahedrons mO .

The hexakis-octahedrons form the *intermediate* planes *i* of the French crystallographers; and as in the combinations in which they occur these planes are usually subordinate to the others, their necessarily small size in drawings often prevents the complete symbol, which is exactly similar to that of Naumann, from being employed in their lettering; for which reason any conventional sign may be used for that purpose, and the exact notation placed beneath the figure: as, " or $\ast = 2O_4$, &c.

Dimetric System.

Monaxial Planes:—

Basal planes, P.

Vertical planes, M.

Diaxial Prismatic Planes:—

Vertical planes alternating in position with those of the ordinary square prisms, D.

Ditetragonal prisms, Dm.

Diaxial Pyramidal Planes:—

Ordinary square-based octahedrons, A,

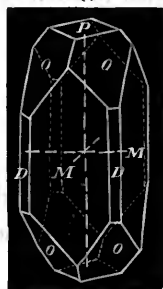
$\frac{1}{m}A$, mA .

Triaxial Planes:—

Octahedral planes alternating in position with the above, O, $\frac{1}{m}O$, mO .

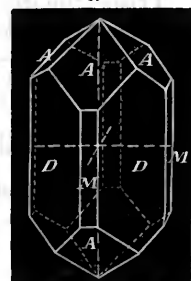
Diocahedrons, mOn .

Fig. 3.



Apophyllite: Faroe.

Fig. 4.



Zircon: Frederiksvärn.

Observations.—In this system, and in those which follow, a fundamental form must be chosen for each mineral as in the usual notations, but with this difference: that whereas the German crystallographers choose invariably pyramidal forms for this purpose, often quite opposed to the crystallization of

the mineral, if not altogether hypothetical, I would select that form most in keeping with the prevailing character of the crystallization; and therefore, sometimes an octahedron with Weiss and his disciples, and sometimes a prism with Lévy and Dufrénoy. It would, however, be still better, as the proposed notation, unlike those of the above crystallographers, has no dependence upon the actual external shape of the fundamental form, to name merely the relative lengths of the axes, thus: *protaxial form* = $\cdot 756 X$ (idocrase); or $\cdot 554 X \cdot 950$, &c., the value before the symbol to be considered the relative length of the vertical axis, and that after the symbol to refer to one of the lateral axes, the other axis being unity. The protaxial forms of the different systems would be then as follows:—monometric system, X ; dimetric and hexagonal systems, xX ; other systems, xXx . In the inclined systems the inclinations of the axes should of course also be given.

Hexagonal System.

Monaxial Planes:—

The basal planes, P .

Di axial Prismatic Planes:—

Vertical planes of ordinary hexagonal prisms, D .

Tri axial Prismatic Planes:—

Vertical planes alternating in position with the above, T .

Tri axial Pyramidal Planes:—

Ordinary hexagonal pyramids, O ,
 $\frac{1}{m}O$, mO .

Tetraxial Planes:—

Hexagonal pyramids alternating in position with the above,
 Y , $\frac{1}{m}Y$, mY .

Dihexagonal pyramids, xYm , $n = xY + 2$, -2 .

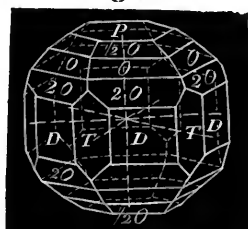
Hemihedral Forms:—

Rhombohedrons, $\frac{O}{2}$, $\frac{\frac{1}{m}O}{2}$, $\frac{mO}{2}$; or conventionally, R .

Scalenohedrons, $\frac{xYmn}{2}$; or $xxxS$; or, in general, simply

xS . In the conventional symbol of these forms, the sign r = the inscribed rhombohedron; and as this is very generally the same as the fundamental rhombohedron, the notation becomes merely xS , and is then, I think, more significative

Fig. 5.



Apatite: Zillerthal.

than Naumann's symbol of R^* . Scalenohedrons, however, are not possessed by more than five or six minerals, and are only abundant in one,—calcareous spar.

Observations.—Rhombohedrons being the more common fundamental forms of this group, Naumann adopts two kindred systems for the notation of the derived forms; one starting with the hexagonal pyramid xP , and the other with the rhombohedron xR , each producing a similar series:— OP , ∞P , $\infty P\infty$,— OR , ∞R , $\infty R\infty$, &c. It is however evident, that in the proposed notation only one series can result, whatever be the fundamental form; for R and S are the only arbitrary symbols; and their employment, in rendering the notation more simple, does not in any way affect the other signs. The triaxial prismatic planes T should be strictly $T_2, 2$; but the figures are quite unnecessary, for these planes being tangents to one of the horizontal axes, it is obvious—the axes crossing each other at 60° —that the other two must be cut at twice the distance. The values also of m and n in the dihexagonal pyramids can only equal $+2, -2$, as otherwise the form Y would result.

Fig. 6.

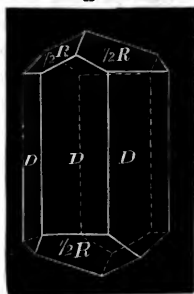
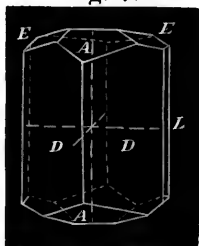
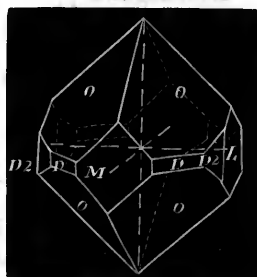
Calc-spar :
Cumberland.

Fig. 7.



Epistilbite: Faroe.

Fig. 8.



Scorodite: Brazil.

Trimetric System.

Monaxial Planes:—

Basal planes, P .

Back and front vertical planes, M .

Side vertical planes, L .

Di axial Prismatic Planes:—

Vertical planes of ordinary rhombic prisms, D , $D_{\frac{1}{m}}$, Dm .

Di axial Pyramidal Planes:—

Inclined planes cutting the shorter horizontal, or frontal axis, A , $\frac{1}{m}A$, mA .

Inclined planes cutting the longer horizontal, or right and left axis, E , $\frac{1}{m}E$, mE .

Triaxial Planes:—

Ordinary rhombic octahedrons, O , $\frac{1}{m}O$, mO .

Intermediate planes, xOx .

Observations.—In this system, in accordance with the general custom, the longer horizontal axis is considered unity. The planes h^2 , h^3 , &c. of the *rhombic prisms* of Lévy and Dufrénoy correspond therefore to the planes D_m^1 of the present notation, because they abut more immediately upon the shorter axis. The planes g^2 , g^3 , &c., on the other hand, are equivalent to Dm . A value following a letter, invariably refers to the relative length of the shorter horizontal axis compared to that of the longer one as unity. Naumann's symbols are, for D_m^1 , $\infty \bar{P}n$; and for Dm , ∞Pn .

The monaxial planes M and L form the vertical faces of rectangular prisms; whilst the biaxial pyramidal planes, xA and xE , produce rectangular octahedrons.

Monoclinic System.

Monaxial Planes:—

Basal planes, P.

Back and front prismatic planes, M.

Right and left prismatic planes, L.

Diaxial Prismatic Planes:—

Ordinary oblique rhombic prisms,

D , D_m^1 , Dm .

Diaxial Pyramidal Planes:—

Upper inclined planes in front, or lower behind, A , $\frac{1}{m}A$, mA .

Upper inclined planes behind, or lower in front, I , $\frac{1}{m}I$, mI .

Inclined planes at the sides, E , $\frac{1}{m}E$, mE .

Triaxial Pyramidal Planes:—

Upper frontal planes of oblique rhombic octahedrons, or lower back planes of the same, O , $\frac{1}{m}O$, mO .

Upper back planes of oblique rhombic octahedrons, or lower planes in front of the same, U , $\frac{1}{m}U$, mU .

Intermediate planes, xOx ; xUx .

Observations.—In this system the orthodiagonal or horizontal axis is made unity; for if the clinodiagonal be selected as such, according to perhaps the more usual custom, the positions of the planes D_m^1 and Dm will be the reverse of what they are in the preceding system. By making, however, in each system that axis unity which passes from left to right, the crystals being in position, those planes which abut the more immediately upon the frontal axis will invariably have the symbol D_m^1 .

Fig. 9.

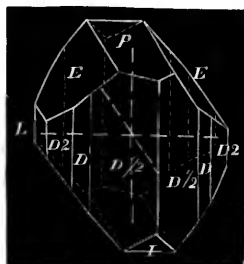


Fig. 10.

*Triclinic System.**Monaxial Planes:—*

P—M—L, as in the preceding system.

Diaxial Prismatic Planes:—

B—B_x; D—D_x.

Diaxial Pyramidal Planes:—

A; AA; E; EE; with their values $\frac{1}{m}$, m , as above.

Triaxial Pyramidal Planes:—

O; OO; U; W; with their values $\frac{1}{m}$, m .

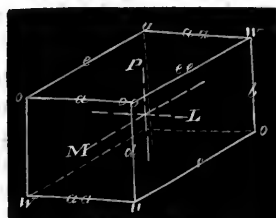
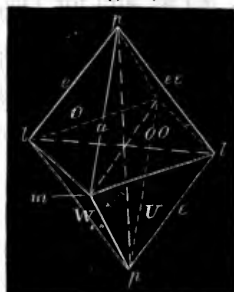


Fig. 11.



In conclusion, we will tabulate the proposed notation, for the sake of reference, with those of Weiss and Rose, Naumann, Lévy and Dufrénoy, and Mr. Griffin. It should be observed, however, that the latter author adopts throughout each crystallographic group one uniform system of three rectangular axes, differing only in regard to their relative lengths*. In the following tables the forms are arranged in accordance with the number of axes which they cut, monaxial forms being placed first.

Monometric System.

E.I.C.	Weiss and Rose.	Nau- mann.	Lévy and Dufrénoy. P. F. the cube.	Griffin.
P	$a : \infty a : \infty a$	$\infty O \infty$	P	P, M, T.
A	$a : a : \infty a$	∞O	b'	MT. PM, PT.
$\frac{1}{m}A$	$a : ma : \infty a$	∞On	b^m	M—T, M+T. P—M, P+M, P—T, P+T
O	$a : a : a$	O	a'	PMT.
$\frac{1}{m}O$	$a : a : \frac{1}{m}a$	mOm	a^m	3P—MT.
mO	$a : a : ma$	mO	$a^{\frac{1}{m}}$	3P+MT.
mOn	$a : \frac{1}{m}a : \frac{1}{n}a$	mOn	$i(=b' b^m b^n)$	6P—MT.

* System of Crystallography, with its Applications to Mineralogy. 1841.

Dimetric System.

E.I.C.	Weiss and Rose.	Naumann.	Lévy and Dufrénoy. P. F. a square prism.	Griffin.
P	$\infty a : \infty a : c$	OP	P	P
M	$\infty a : a : \infty c$	$\infty P\infty$	M	M ; T
D	$a : a : \infty c$	∞P	h'	MT
D_m	$a : na : \infty c$	∞P_n	h^m	M—T, M+T
A	$a : \infty a : c$	$P\infty$	b'	} PrM, PrT
$\frac{1}{m}A$	$a : \infty a : \frac{1}{m}c$	$nP\infty$	b^m	
mA	$a : \infty a : mc$	$mP\infty$	$b^{\frac{1}{m}}$	
O	$a : a : c$	P	a'	} PrMT
$\frac{1}{m}O$	$a : a : \frac{1}{m}c$	nP	a^m	
mO	$a : a : mc$	mP	$a^{\frac{1}{m}}$	
mOn	$a : na : mc$	mP_n	$i(=b' b^{\frac{1}{m}} h^{\frac{1}{m}})$	PrMyTz . PrMzTy

Hexagonal System.

E.I.C.	Weiss and Rose.	Naumann.	Lévy and Dufrénoy. P.F. hexagonal prism.	Griffin.
P	$\infty a : \infty a : \infty a : c$	OP	P	P
D	$a : a : \infty a : \infty c$	∞P	M	M, $M2T\frac{1}{3}$
T	$2a : a : 2a : \infty c$	$\infty P2$	h'	T, $M\frac{1}{3}T2$
O	$a : a : \infty a : c$	P	b'	} PrM, PrM2T $\frac{1}{3}$
$\frac{1}{m}O$	} $a : a : \infty a : mc$	nP	b^m	
mO			$b^{\frac{1}{m}}$	
Y	$2a : a : 2a : c$	$P2$	a'	} PrT, PrM $\frac{1}{3}T2$
$\frac{1}{m}Y$	} $2a : a : 2a : mc$	$nP2$	a^m	
mY			$a^{\frac{1}{m}}$	
xYx, x	$a : na : pa : mc$	xPm, n	$b^{2m} b^{2m} h^x$	$3p_x m_y tz$
—		—		
xR	$\left\{ \begin{array}{l} \frac{1}{2}(a : a : \infty a : mc) \\ \frac{1}{2}(a' : a' : \infty a : mc) \end{array} \right\}$	xR	Rx
$xxrS$	$\left\{ \begin{array}{l} \frac{1}{2}(a : na : pa : mc) \\ \frac{1}{2}(a' : na' : pa : mc) \end{array} \right\}$	xR^x	Sr

Trimetric System.

E.I.C.	Weiss and Rose.	Naumann.	Lévy and Dufrénoy. P.F. a rectangular prism*.	Griffin.
P	$\infty a : \infty b : c$	OP	P	P
M	$a : \infty b : \infty c$	$\infty \bar{P}\infty$	M	M
L	$\infty a : b : \infty c$	$\infty \check{P}\infty$	T	T
D	$a : b : \infty c$	∞P	h'	MxT
$D\frac{1}{m}$	$\left. \begin{array}{l} \\ \end{array} \right\} a : mb : \infty c \left\{ \right.$	$\infty \bar{P}n$	h^m	
Dm		$\infty \check{P}n$	$\frac{1}{h^m}$	
A	$a : \infty b : c$	$\bar{P}\infty$	b'	PxM
$\frac{1}{m}A$	$\left. \begin{array}{l} \\ \end{array} \right\} a : \infty b : mc$	$m\bar{P}\infty$	b^m	
mA			$\frac{1}{b^m}$	
E	$\infty a : b : c$	$\check{P}\infty$	d'	PxT
$\frac{1}{m}E$	$\left. \begin{array}{l} \\ \end{array} \right\} \infty a : b : mc$	$m\check{P}\infty$	d^m	
mE			$\frac{1}{d^m}$	
O	$a : b : c$	P	a'	$PxMyTz$
$\frac{1}{m}O$	$\left. \begin{array}{l} \\ \end{array} \right\} a : b : mc$	mP	a^m	
mO			$\frac{1}{a^m}$	
xOx	$ma : nb : c$	mPn	$i, i', \&c.$	

Monoclinic System.

E.I.C.	Weiss and Rose.	Naumann.	Lévy and Dufrénoy. P.F. an oblique rhombic prism.	Griffin.
P	$\infty a : \infty b : c$	OP	P	$\frac{1}{2}PxM$
M	$a : \infty b : \infty c$	$\infty P\infty$	h'	M
L	$\infty a : b : \infty c$	$[\infty P\infty]$	g'	T
D	$a : b : \infty c$	∞P	M	MxT
$D\frac{1}{m}$	$a : mb : \infty c$	$\pm \infty Pn$	h^m	
Dm	$ma : b : \infty c$	$[\pm \infty Pn]$	g^m	
A	$a : \infty b : c$	$-P\infty$	o'	$\frac{1}{2}PxM, Zn.$
$\frac{1}{m}A$	$\left. \begin{array}{l} \\ \end{array} \right\} a : \infty b : mc$	$-mP\infty$	o^m	
mA			$\frac{1}{o^m}$	

* If a right rhombic prism be chosen as the primary form, M in the proposed notation will equal h' ; L, g' ; D, M; $D\frac{1}{m}$, h^m ; Dm , g^m ; A, a' ; E, e' ; and O, b' .

Monoclinic System (continued).

E.I.C.	Weiss and Rose.	Naumann.	Lévy and Dufrénoy. P.F. an oblique rhom- bic prism.	Griffin.
I	$a' : \infty b : c$	$+P\infty$	a'	$\frac{1}{2}PxM, Z_s$
$\frac{1}{m}I$	$a' : \infty b : mc$	$+mP\infty$	a^m	
mI			$\frac{1}{a^m}$	
E	$\infty a : b : c$	$\mp [P\infty]$	e'	$\frac{1}{2}PxT, Z_{e \text{ or } Z_w}$
$\frac{1}{m}E$	$\infty a : b : mc$	$\mp [mP\infty]$	e^m	
mE			$\frac{1}{e^m}$	
O	$a : b : c$	$-P$	d'	$\frac{1}{2}Px My T_z, Z_{ne \text{ or } Z_{nw}}$
$\frac{1}{m}O$	$a : b : mc$	$-mP$	d^m	
mO			$\frac{1}{d^m}$	
U	$a' : b : c$	$+P$	b'	$\frac{1}{2}Px My T_z, Z_{se \text{ or } Z_{sw}}$
$\frac{1}{m}U$	$a' : b : mc$	$+mP$	b^m	
mU			$\frac{1}{b^m}$	
xOx	$ma : nb : c$	$-mP_n$	$i, \&c.$	
xUx	$ma' : nb : c$	$+mP_n$	$i, \&c.$	

Triclinic System.

E.I.C.	Weiss and Rose.	Naumann.	Lévy and Dufrénoy. P.F. pr. obl. non symétrique.	Griffin.
P	$\infty a : \infty b : c$	OP	P	$\frac{1}{4}Px My T_z$
M	$a : \infty b : \infty c$	$\infty \bar{P}\infty$	M	M
L	$\infty a : b : \infty c$	$\infty \check{P}\infty$	T	T
B	$a : b' : \infty c$	$\infty'P$	g'	$\frac{1}{2}MxT, nw.$
D	$a : b : \infty c$	$\infty P'$	h'	$\frac{1}{2}MxT, ne.$
A	$a : \infty b : c$	$\bar{P}'\infty$	d'	$\frac{1}{4}Px My T_z, \text{ the individual plane being determined by the zone.}$
AA	$a' : \infty b : c$	$\bar{P}_1\infty$	c^*	
E	$\infty a : b' : c$	$\check{P}'\infty$	b'	
EE	$\infty a : b : c$	$\check{P}_1\infty$	f'	
O	$a : b' : c$	$'P$	e'	
OO	$a : b : c$	P'	o'	
U	$a' : b : c$	P_1	a'	
W	$a' : b' : c$	$_1P$	i'	

* In Dufrénoy's primary form, the plane c' is sometimes made an upper back plane, and at other times an upper front one. When the latter is the case, as in his figures of albite (*Traité de Min.*, Atlas, pl. 166 to 171), c will equal A , and d AA, of the proposed notation.

XLII. *On a peculiar Fibre of Cotton which is incapable of being Dyed.* By WALTER CRUM, Esq., F.R.S., Vice-President of the Philosophical Society of Glasgow*.

IN the month of May last, Mr. Thomson of Primrose received from Mr. Daniel Kœchlin of Mülhausen some specimens of a purple ground printed calico, each of them containing a portion of cotton which was white, although subjected to the same treatment by which the rest of the cloth, and even the threads which crossed the white one, was uniformly dyed. The white part of the thread was usually thicker than the rest, and little more than a quarter of an inch long. The whole fabric had been thoroughly bleached before printing, so that it contained no grease or other impurity that could resist the colouring matter.

White specks like these are not unknown or undreaded among the printers of calicoes in this country. M. Kœchlin mentions that the cotton of which they are formed is known by the name of *coton mort*, and here also it is called *dead cotton*. M. Kœchlin has been the first, I believe, to suggest that it may consist of unripe cotton, and that its fibre may be solid, wanting the hollow of the more perfect fibre. He adds, that if such should prove to be the case, its behaviour with colouring matters may affect materially the question of the mechanical or chemical nature of the union of cotton with its dye. Mr. Thomson did me the honour to transmit me the specimens for examination.

The ordinary cotton fibre, it will be remembered, is described by Mr. Thomson in the memoir where its form was first made known†, as a tube, originally cylindrical, but which collapses in drying. It has then the appearance of two small tubes joined together, so that a transverse section of the filament resembles in some degree a figure of 8. Until full maturity the cylinder is distended with water, in which bubbles of air are often distinguishable.

On placing a few of the fibres of the *coton mort* under the microscope, I found them to consist of very thin and remarkably transparent blades, some of which are marked or spotted, while others are so clear as to be almost invisible except at the edges. These fibres are readily distinguished from those of ordinary cotton by their perfect flatness, without the vestige of a cavity, even at the sides, and by their uniform

* From the Proceedings of the Philosophical Society of Glasgow, 1848–49, and read before that Society April 25, 1849.

† Annals of Philosophy for June 1834. Lately reprinted in the Classical Museum, No. 20; and in Liebig's *Annalen* for January 1849.

as well as great transparency. They are often broader, too, than the usual fibre, and they show numerous folds, both longitudinal and transverse; but they are never twisted into the corkscrew form of the ordinary fibre.

It occurred to me that cotton of this description might be detected among the wool as it is imported. I searched accordingly for any portions that had a different appearance from the rest; and having collected and examined them, I found one sort whose filaments had exactly the appearance under the microscope of the *coton mort* in the pattern of M. Kœchlin. It occurs in the form of a small matted tuft of a shining silky lustre, and usually contains in its centre the fragment of a seed, or perhaps an abortive seed. It consists of short fibres having little tenacity. Specimens of it are found in abundance among the motes or hard portions, called droppings, rejected by the picking machine in the preparation for spinning. Small tufts of it, however, do occasionally pass the sifting process of the picking machine; and then, their fibres being too short to be teased out in the carding engine, or drawn into threads in the subsequent operations of cotton spinning, remain as minute lumps or knots upon the threads of better wool.

Although the microscopic appearance of the fibre in question is that of a flat single blade, the cellular character of the tissue scarcely admits of such a formation. We must rather suppose that, like the healthy unripe cotton fibre, it was originally an elongated cell or tube filled with liquid; that the seed around which it began to grow had died soon after its formation, while the fibres which clothed it were yet soft and pliable; and that the flattening, and perhaps growing together of the sides of the tube, was occasioned by the pressure from the increasing crop of cotton attached to the numerous other seeds confined in the same pod.

To explain the bearing of this peculiar structure upon the question whether cotton wool and colouring matters form together a true chemical compound, or are held together by a merely mechanical power, I must quote a passage from a memoir on this subject which I read to the Philosophical Society six years ago, and refer to the memoir itself for additional illustrations.

“In many of the operations of dyeing and calico-printing, the mineral basis of the colour is applied to the cotton in a state of solution in a volatile acid. This solution is allowed to dry upon the cloth, and in a short time the salt is decomposed, just as it would be, in similar circumstances, without the intervention of cotton. During the decomposition of the

salt its acid escapes, and the metallic oxide adheres to the fibre so firmly as to resist the action of water applied to it with some violence. In this way does acetate of alumina act; and, nearly in the same manner, acetate of iron. The action here can only be mechanical on the part of the cotton; and the adherence, as I shall endeavour to show, confined to the interior of the tubes of which wools consist, or of the invisible passages which lead to it. The metallic oxide permeates these tubes in a state of solution; and it is only when its salt is there decomposed, and the oxide precipitated and reduced to an insoluble powder, that it is prevented from returning through the fine filter in which it is then enclosed.

“When the piece of cotton, which, in this view, consists of bags lined inside with a metallic oxide, is subsequently dyed with madder or logwood, and becomes thereby red or black, the action is purely one of chemical attraction between the mineral in the cloth and the organic matter in the dye-vessel, which, together, form the red or black compound that results; and there is no peculiarity of a chemical nature, from the mineral constituent being previously connected with the cotton.”

To produce the purple dye of M. Kœchlin's pattern, the cloth has first to be impregnated with iron. For this purpose it is made to imbibe a weak solution of proto-acetate of iron, and afterwards dried. By exposure to the air for some days the salt is decomposed. Its acetic acid evaporates, and the oxide of iron, then become peroxide, remains in the fibre. The cloth is afterwards subjected to severe washings in hot and cold water, but its iron is not removed; and the question is, How is it retained in connexion with the cotton? Mechanically, as I maintain, and probably in the interior of its hollow fibre, which it entered in a state of solution, and within which it was precipitated. Others, as I have already stated, are of opinion, after Bergman, that the combination is a chemical one; and so fully is that view carried out by my friend Professor Runge of Oranienburg, in his ingenious and excellent work on the Chemistry of Dyeing*, that he assumes coloured cottons to be combinations of what he calls cottonic acid with the various bases, in definite, and even in multiple proportions. Thus a very pale shade of buff from oxide of iron is called *percottonate of iron*; another is called *bicottonate of iron*; and still deeper shades, *cottonate* and *basic cottonate of iron*.

But the new fibre, by the same treatment, is incapable of retaining the iron mordant, and yet both fibres have the same chemical composition and the same ultimate structure. The only difference is, that one is shaped into tubes or bags capable

* *Farbenchemie*. 2 vols. Berlin, 1832 and 1845.

of holding all matters which are insoluble in water, that is, all bodies which can be caught upon a filter, while the other is possessed of no such enclosure.

I take this opportunity, in reply to a review of my first memoir on this subject, by M. Persoz, in his remarkable work *Traité de l'Impression des Tissus*, of explaining that I attribute to an attraction of surface those cases of dyeing where pure cotton, by mere immersion, is enabled to decompose the solid matters in solution, and to withdraw them from the solvent. Such is the case with the solution of deoxidized indigo in lime, with the plumbite of lime, with the various salts of tin, and many other solutions. Cotton, as I have stated, acts in these cases like charcoal and other porous bodies, and I have seen no reason to confine the attraction in question to the internal surface of the cotton fibre.

But I have not ranked the aluminous mordant among the class of bodies so attracted; because cotton, when immersed in a solution of acetate of alumina, has not the power of separating its base. That solution must be applied to cotton and dried in it; and then the alumina only adheres, or loses the power of being washed away, in proportion as the acetic acid is removed by evaporation. I could see here no chemical decomposition effected by the cotton wool, for the same salt may be decomposed by evaporation in a glass vessel. In this case I have represented the alumina as being held in the interior of the fibre, just as sand may be held in a bag whose interstices are too narrow to allow its particles to pass.

M. Persoz has remarked, however, that by evaporating a solution of acetate of alumina in a glass vessel we do not so thoroughly decompose it as by drying the same substance upon calico. This I also have observed; and although I have been accustomed to ascribe the difference to the more extensive division and exposure of the salt upon cotton, I have no proof, and shall not deny, that the presence of cotton at a particular stage of the evaporation may accelerate the decomposition of the salt, and that its fibres may thus attract a portion of alumina over their whole surface. If this modification of the view I had given be correct, the action of the *coton mord* proves at least that colouring matter adhering outside is not so permanent as that which is held within the fibre of the mature cotton.

Neither view gives any countenance to the chemical theory. Porous bodies are well known to attract, and even to decompose, without chemically combining with the substances they

precipitate. Accordingly, none of the oxides are changed either in colour or in chemical character by their union with cotton. The hydrated oxide of copper, for example, precipitated upon calico, becomes carbonate, or arsenite, when exposed to carbonic or arsenious acid. The protoxide of iron changes speedily in the air into the red sesquioxide, and that again may be converted into prussian blue, or into a black or purple lake—every new compound, if it only be insoluble, adhering firmly to the wool.

XLIII. On the Theory of the Tides.

By the Rev. BRICE BRONWIN.

[Concluded from p. 270.]

IF we now take account of a second planet, marking the quantities relative to it with an accent, we have

$$y = F_2 \cos 2(\phi - \beta_2) + F'_2 \cos 2(\phi' - \beta'_2) + F_1 \cos (\phi - \beta_1) + F'_1 \cos (\phi' - \beta'_1).$$

But $F_1 \cos (\phi - \beta_1) + F'_1 \cos (\phi' - \beta'_1)$ is very small in our seas, and perhaps generally so; therefore, neglecting it,

$$y = F_2 \cos 2(\phi - \beta_2) + F'_2 \cos 2(\phi' - \beta'_2).$$

We cannot affirm that β'_2 is exactly equal to β_2 , though we may suppose their difference to be very small. Make

$$\psi - \psi' + \beta_2 - \beta'_2 = \Delta,$$

or

$$\psi + \beta_2 = \psi' + \beta'_2 + \Delta;$$

then

$$y = F_2 \cos 2(\phi' - \beta'_2 - \Delta) + F'_2 \cos 2(\phi' - \beta'_2).$$

$$\begin{aligned} \frac{dy}{dt} = & - \left(2p'n - 2 \frac{d\Delta}{dt} \right) F_2 \sin 2(\phi' - \beta'_2 - \Delta) - 2p'n F'_2 \sin 2(\phi' - \beta'_2) \\ & + \frac{dF_2}{dt} \cos 2(\phi' - \beta'_2 - \Delta) + \frac{dF'_2}{dt} \cos 2(\phi' - \beta'_2). \end{aligned}$$

If we suppose the first planet to be the sun and the second the moon, $\frac{dF_2}{dt}$ is very small compared with $\frac{dF'_2}{dt}$, and may be neglected. Moreover the variable part of F'_2 contains only very small quantities, and in $\frac{dF'_2}{dt}$ they will be multiplied by the small quantity ψ' ; we shall therefore neglect $\frac{dF'_2}{dt}$. Then

$$\frac{dy}{dt} = 0 \text{ gives}$$

$$\left\{ \left(2p'n - 2 \frac{d\Delta}{dt} \right) F_2 \cos 2\Delta + 2p'n F'_2 \right\} \sin 2(\phi' - \beta'_2) \\ - \left(2p'n - 2 \frac{d\Delta}{dt} \right) F_2 \sin 2\Delta \cos 2(\phi' - \beta'_2) = 0,$$

and

$$\tan 2(\phi' - \beta'_2) = \tan 2(nt + \varpi - \psi' - \beta'_2) \\ = \frac{\left(2p'n - 2 \frac{d\Delta}{dt} \right) F_2 \sin 2\Delta}{2p'n F'_2 + \left(2p'n - 2 \frac{d\Delta}{dt} \right) F_2 \cos 2\Delta},$$

which gives the time of high water.

If we take account of the two small terms $F_1 \cos(\phi - \beta_1)$, $F'_1 \cos(\phi' - \beta'_1)$, we may substitute in their differentials the value of ϕ' given by the preceding formula, and thus correct it; but the correction would be too complicated for practical purposes. Dividing the numerator and denominator by

$$2p'n - 2 \frac{d\Delta}{dt},$$

it becomes

$$\tan 2(\phi' - \beta'_2) = \frac{F_2 \sin 2\Delta}{F'_2 \delta + F_2 \cos 2\Delta},$$

where

$$\delta = \frac{p'n}{p'n - \frac{d\Delta}{dt}} = \frac{1}{1 - \frac{d\Delta}{ndt}} = 1 + \frac{d\Delta}{ndt}$$

very nearly, since $p'n = n - cv'$.

We have found

$$d\psi = dz \left(1 - \frac{1}{2} \sin^2 o \cos 2z \right);$$

therefore, making d' and o the same, since their difference is small,

$$d\psi' = dz' \left(1 - \frac{1}{2} \sin^2 o \cos 2z' \right),$$

and

$$d(\psi - \psi') = dz - dz' - \frac{1}{2} \sin^2 o (dz \cos 2z - dz' \cos 2z').$$

Also

$$vdt = dz (1 - 2e \cos(z - \pi));$$

whence

$$\frac{dz}{dt} = v(1 + 2e \cos(z - \pi)), \quad \frac{dz'}{dt} = v'(1 + 2e' \cos(z' - \pi')).$$

Consequently

$$\frac{d(\psi - \psi')}{dt} = v - v' - 2v'e' \cos(z' - \pi') - \frac{1}{2} \sin^2 o (v \cos 2z - v' \cos 2z'),$$

neglecting the terms containing the very small quantities ve and $\frac{1}{2} \sin^2 o v'e'$.

Again, by (17.),

$$\begin{aligned} \frac{d(\epsilon_2 - \epsilon'_2)}{dt} &= \frac{1}{2} \sin^2 o \left(\cos 2z \frac{dz}{dt} - \cos 2z' \frac{dz'}{dt} \right) - 2e \frac{dz}{dt} \cos(z - \pi) \\ &\quad + 2e' \frac{dz'}{dt} \cos(z' - \pi') = \frac{1}{2} \sin^2 o (v \cos 2z - v' \cos 2z') \\ &\quad + 2v'e' \cos(z' - \pi'), \end{aligned}$$

neglecting the same small quantities as before. And

$$\frac{d(\epsilon_2 - \epsilon'_2)}{dt} = \frac{d(\beta_2 - \beta'_2)}{dt}$$

very nearly; therefore

$$\frac{d\Delta}{ndt} = \frac{d(\psi - \psi')}{ndt} + \frac{d(\beta_2 - \beta'_2)}{ndt} = \frac{v - v'}{n} = -\frac{v'}{n}$$

very nearly. Hence we have

$$\delta = 1 - \frac{v'}{n}.$$

It would be better to divide the numerator and denominator of the value of $\tan 2(\phi' - \beta'_2)$ by F_2 , and reduce $\frac{F'_2}{F_2} \delta$ to its simplest form.

If we wish to make $\delta\omega$ a complete variation relative to r and ϖ , the equation of condition is, leaving out the terms containing (s) as before, these being insensible,

$$\frac{d}{dr} \left\{ r^2 \sin^2 \theta \frac{d^2 v}{dt^2} + 2nr^2 \sin \theta \cos \theta \frac{du}{dt} \right\} = -2nr \sin^2 \theta \frac{d^2 v}{d\varpi dt}.$$

Make

$$\frac{du}{dt} = A \sin i(\phi - \epsilon), \quad \sin \theta \frac{dv}{dt} = B \cos i(\phi - \epsilon),$$

A and B being functions of r and θ without t . By substitution and dividing the result by $n \sin \theta \sin i(\phi - \epsilon)$, we have

$$\frac{d}{dr} \{ -ipr^2 B + 2r^2 \cos \theta A \} = 2irB,$$

or

$$ipr \frac{dB}{dr} - 2r \cos \theta \frac{dA}{dr} + 2i(p+1)B - 4 \cos \theta A = 0.$$

We shall neglect v , then $p=1$, and the last becomes

$$ir \frac{dB}{dr} - 2r \cos \theta \frac{dA}{dr} + 4iB - 4 \cos \theta A = 0. \quad (19.)$$

To make $\delta\omega$ a complete variation relative to r and θ , we must have

$$\frac{d}{dr} \left\{ r^2 \frac{d^2 u}{dt^2} - 2nr^2 \sin \theta \cos \theta \frac{dv}{dt} \right\} = -2nr \frac{d}{d\theta} \left(\sin^2 \theta \frac{dv}{dt} \right).$$

Substituting for $\frac{d^2 u}{dt^2}$, &c., and dividing by $n \cos i(\phi - \epsilon)$, we find

$$\frac{d}{dr} \left\{ ipr^2 A - 2r^2 \cos \theta B \right\} = -2r \frac{d}{d\theta} (\sin \theta B),$$

or

$$ipr \frac{dA}{dr} - 2r \cos \theta \frac{dB}{dr} + 2ipA - 4 \cos \theta B + 2 \frac{d}{d\theta} (\sin \theta B) = 0.$$

Making $p=1$, and putting for $\frac{d}{d\theta} (\sin \theta B)$ its value from (e), there results

$$ir \frac{dA}{dr} - 2r \cos \theta \frac{dB}{dr} + \left(4i - \frac{4}{i} \sin^2 \theta \right) A - 4 \cos \theta B = 0. \quad (20.)$$

Make

$$A = \Sigma C r^m, \quad B = \Sigma D r^m.$$

By the substitution of these values in (19.) and (20.), we find

$$\left. \begin{aligned} (m+4)iD - (2m+4) \cos \theta C &= 0, \\ (2m+4) \cos \theta iD - (i^2(m+4) - 4 \sin^2 \theta) C &= 0. \end{aligned} \right\} \quad (21.)$$

Eliminate $\frac{D}{C}$ between these, and there results

$$i^2(m+4)^2 - (2m+4)^2 \cos^2 \theta - (m+4)4 \sin^2 \theta = 0.$$

Thus (19.) and (20.) are rendered identical. Let the roots of the last equation be m_1 and m_2 , and we have

$$A = C_1 r^{m_1} + C_2 r^{m_2}, \quad B = D_1 r^{m_1} + D_2 r^{m_2}.$$

When $i=2$, we find

$$m_1 = -3, \quad m_2 = -\frac{4}{\sin^2 \theta}.$$

When $i=1$,

$$m_1=0, \quad m_2=-4-\frac{4 \cos^2 \theta}{1-4 \cos^2 \theta}.$$

The first value of m_2 is infinite negative at the pole, and the second is so likewise when $\cos \theta = \frac{1}{2}$, or in the latitude of 30° .

If, therefore, r be less than unity, r^{m_2} is infinite at both places; and if r be greater than unity, it is nothing. This is inadmissible, since we can give to r any numerical value we please. We must therefore have $C_2=0$, $D_2=0$, or the constant arbitraries by which the values of these quantities are multiplied must be nothing. Thus we have

$$A=C_1 r^{m_1}, \quad B=D_1 r^{m_1};$$

and therefore

$$C_1 \text{ or } C = A r^{-m_1}, \quad D_1 \text{ or } D = B r^{-m_1}.$$

These values, substituted in the first of (21.), give

$$(m+4)iB - (2m+4) \cos \theta A = 0.$$

If by means of this we eliminate B from the equations

$$\frac{d}{d\theta} (\sin \theta A) - iB = 0, \quad i \frac{d}{d\theta} (\sin \theta B) - (i^2 - 2 \sin^2 \theta) A = 0,$$

given in the first paper, we find

$$\begin{aligned} \sin \theta \frac{dA}{d\theta} - \frac{m}{m+4} \cos \theta A &= 0, \\ \frac{2m+4}{m+4} \cdot \frac{d}{d\theta} (\sin \theta \cos \theta A) - (i^2 - 2 \sin^2 \theta) A &= 0. \end{aligned}$$

Let $i=2$, $m=-3$; and both these reduce to

$$\sin \theta \frac{dA}{d\theta} + 3 \cos \theta A = 0,$$

which, integrated, gives

$$A = A_2 = \frac{a_2}{\sin^3 \theta}.$$

This will be found upon trial to be a particular integral of (7.).

Let $i=1$, $m=0$; and both the above reduce to $\frac{dA}{d\theta} = 0$, which gives $A = A_1 = a_1$, which is also a particular integral of (7.) in this case. These results serve to confirm the legitimacy of the process; and since each of the above equations leads to the same result, the confirmation is in a manner doubled.

The other particular integral of (7.), when $i=2$, found by means of the above, is

$$\frac{b_2 \cos \theta}{\sin^3 \theta} (2 + \sin^2 \theta);$$

so that the complete integral is

$$A_2 = \frac{a_2}{\sin^3 \theta} + \frac{b_2 \cos \theta}{\sin^3 \theta} (2 + \sin^2 \theta).$$

This is the form in which I first found it. But each of these particular integrals is infinite at the pole. Make

$$b_2 = -\frac{1}{2} a_2 + c_2;$$

then

$$A_2 = a_2 \left(\frac{1 - \cos \theta}{\sin^3 \theta} - \frac{\cos \theta}{2 \sin \theta} \right) + \frac{c_2 \cos \theta}{\sin^3 \theta} (2 + \sin^2 \theta).$$

The second of these particular integrals is infinite at the pole; we must therefore make $c_2 = 0$. Then

$$\begin{aligned} A_2 &= a_2 \left(\frac{1 - \cos \theta}{\sin^3 \theta} - \frac{\cos \theta}{2 \sin \theta} \right) = \frac{a_2}{2 \sin \theta} \left(\frac{1}{\cos^2 \frac{\theta}{2}} - \cos \theta \right) \\ &= \frac{a_2}{2 \sin \theta} \left(\frac{1}{\cos^2 \frac{\theta}{2}} - 1 + 2 \sin^2 \frac{\theta}{2} \right) = \frac{a_2 \sin^2 \frac{\theta}{2}}{2 \sin \theta} \left(\frac{1}{\cos^2 \frac{\theta}{2}} + 2 \right) \\ &= a_2 \frac{\sin \frac{\theta}{2}}{\cos^3 \frac{\theta}{2}} \left(1 + 2 \cos^2 \frac{\theta}{2} \right), \end{aligned}$$

which is also a particular integral of (7.), and the value before given to A_2 .

From (19.) and (20.) we may find equations containing A only, or B only. By elimination and differentiation, and then eliminating again, we shall easily find

$$(i^2 - 4 \cos^2 \theta) r^2 \frac{d^2 A}{dr^2} + (9i^2 - 4 - 16 \cos^2 \theta) r \frac{dA}{dr} + (16i^2 - 16) A = 0$$

$$(i^2 - 4 \cos^2 \theta) r^2 \frac{d^2 B}{dr^2} + (9i^2 - 4 - 16 \cos^2 \theta) r \frac{dB}{dr} + (16i^2 - 16) B = 0.$$

The integrals of these are obtained by assuming $A = Cr^m$, $B = Dr^m$, and each of them gives the same values of m , and these will be found the same as those before obtained.

Suppose, for the sake of illustration, that there is only one

planet to raise the water; we see from what has been done in these papers that the passage of the planet over the meridian and the time of high water will not necessarily coincide, and that the retardation or acceleration of the tide is a necessary consequence of the mathematical theory itself. Moreover, we see that the retardation or acceleration is not a constant quantity; but the variable part of it is small, and may be insensible.

The four arbitraries a_2, a_1, k_2, k_1 , which have been introduced by integration, depend upon the depth of the sea, and the form of the shores; or, in other words, they depend upon the closed surface of the water, and must be determined for each particular place by observation. Hence the theory and observation will not be at variance, as heretofore, with regard to the height of the tide at any particular place, or in any particular latitude, when these arbitraries are properly determined.

The variation $\delta\omega$ has been made a complete variation relative to the quantities θ, ϖ , and r ; but the terms depending on ν have been neglected; not necessarily, but for the sake of simplicity. It is possible, however, small as this quantity is, that it might make a sensible alteration in the form at least of the coefficients as functions of θ ; but this is a matter of no practical importance.

Heretofore the direct action of the planet has alone been considered in this theory; whereas the simple consideration that the water will turn round an island or headland, and quite change or even reverse its course, and this perhaps in a very short time, and with little or no variation in the velocity, in despite of the action of the planet, shows how very small a part of the cause its direct action is. The height of the tide, therefore, in particular places has been made to depend on a cause which produces directly scarcely any part of it; and the horizontal displacements and velocities, which ought chiefly to be considered, have been entirely neglected. This has no doubt been owing to the extreme difficulty of taking account of them.

The difficulty experienced in putting the coefficient of $\cos(\phi - \beta_1)$ under a suitable form, owing to our not being able to estimate with sufficient precision the relative magnitudes of the quantities which it contains, renders it very difficult to say what alteration this theory will make in the height of the tide as it depends on the declinations of the planets, and it is possible that the coefficient may vary greatly in different places.

It will be seen that in the coefficients of both $\cos 2(\phi - \beta_2)$ and $\cos(\phi - \beta_1)$ this theory makes a considerable alteration as

to the manner in which the variable quantities enter them. And if we were to take account of the powers and products of the displacements of the second order, a further change would be produced, and one which might be very sensible. I may perhaps give the subject a brief consideration at some future opportunity, if not prevented.

Gunthwaite Hall, near Barnsley, Yorkshire,
October 5, 1849.

XLIV. *On the Compounds of the Halogens with Phosphorus.*

By J. H. GLADSTONE, *Ph.D.**

DURING the course of my recent examination of the compounds of phosphorus and nitrogen, I had occasion to prepare considerable quantities of pentachloride of phosphorus; and some observations which I incidentally made upon that substance, induced me to undertake a fuller investigation into such combinations of phosphorus with the halogens.

It is well known that if chlorine gas be passed over phosphorus, they combine energetically, forming in the first instance a limpid liquid, which is a combination of three atoms of chlorine with one of phosphorus. If the proper amounts of bromine and phosphorus be brought into contact, a similar tribromide is the result. If one part of phosphorus and twelve parts of iodine be mixed, a red, easily fusible solid results, which appears to be an analogous compound. And again, if a larger quantity of chlorine, or bromine, or iodine, be employed, crystalline bodies are produced, which are combinations of five atoms of the halogen with one of phosphorus. The same penta-compounds result when an additional quantity of the halogen is added to the substance containing three atoms. I have not succeeded in bringing about similar combinations between cyanogen and phosphorus: in fact, this element may be distilled unchanged in an atmosphere of cyanogen gas.

These compounds form a very distinctly-marked class of bodies. They are neither acid nor basic; and they are resolved by water into the hydracids of the halogens, that is, hydrochloric, hydrobromic, or hydriodic acid, and phosphorous, or phosphoric acid,—according to the number of atoms of the element replaced by oxygen. In these respects they differ from the combinations of sulphur with phosphorus, which have acid properties, are not readily decomposed by water, and are in fact strictly analogous to the oxygen compounds.

* Communicated by the Author.

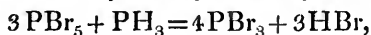
It is also well known, that if the combination between phosphorus and the halogen be made by means of a metallic salt that yields up its electro-negative element readily, or in other words, if phosphorus be distilled with the chloride, bromide, or iodide of mercury, it is the ter-compound that results. In this manner also Davy produced a ter-fluoride. Canedella* states that if cyanide of mercury and phosphorus be heated together, combination ensues, frequently with explosive violence, and a sublimate is formed resolvable by water into phosphorous and hydrocyanic acids. On repeating his experiment I did obtain a sort of explosion, and a minute quantity of a substance which answered somewhat to the description given. A subsequent attempt, however, was quite unsuccessful. The uncertainty attending the preparation, together with the high probability that poisonous exhalations would arise from such cyanides when exposed to the air, rendered me disinclined to pursue the investigation further.

In order to prepare one of these ter-compounds by means of a metallic salt, it is not necessary that the salt should be capable of decomposition by heat alone. I have prepared ter-chloride of phosphorus by making use of the chlorides of iron and copper; and doubtless other metallic chlorides might be substituted. Chloride of lead, however, I found not to be decomposed by phosphorus.

The comparative weakness with which the two additional atoms of the pentachloride of phosphorus are held in combination, is evident from the ease with which compounds are obtained, where these two atoms of chlorine are substituted by sulphur and oxygen, as already made known by Serullas and Wurtz; but the readiness with which the penta-compounds are reduced to the ter-compounds has never, I think, been pointed out. If the chloride, bromide, or iodide of phosphorus, containing five atoms of the halogen, be heated in contact with fresh phosphorus, chemical action is instituted, and the whole is reduced to the compound containing three atoms. With the bromide the application of extraneous heat is quite unnecessary: with the iodide, the reduction will proceed even to the formation of a compound containing a much smaller amount of iodine, but of a totally different character to the bodies now under consideration. Upon this may be founded a good method for obtaining the terchloride and terbromide, as their freedom from admixture with the higher combination can then be depended upon: sufficient phosphorus must be employed to reduce the whole, and the liquid may be distilled of perfect purity. Similarly, it has been heretofore observed,

* *Annal. der Pharm.*, xviii. 70.

that the pentachloride of phosphorus is reduced to the terchloride by the action of phosphuretted hydrogen; and this again is completely decomposed, if the current of gas be continued. If the yellow crystals of pentabromide be subjected to the same treatment, a liquid compound is instantly produced, whilst acid fumes are evolved; and the long-continued action of the phosphuretted hydrogen gas causes the entire disappearance of the liquid, yellow phosphorus being left in its place. The reaction is evidently the same as in the case of the chloride, and may be expressed by the two formulæ,—



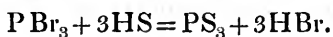
and



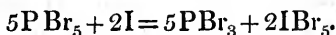
Although phosphuretted hydrogen is thus capable of reducing these combinations of phosphorus with the halogens, pure hydrogen gas has not the slightest effect upon the pentachloride of phosphorus even at the subliming point of that substance; nor does it produce any alteration in the pentabromide,—at least not at the ordinary temperature. Heat alone, however, will effect the reduction of this latter compound, provided a stream of dry air be passed over it: the experiment is conveniently made in a water-bath at $100^\circ \text{C}.$, when bromine passes off, and the liquid terbromide remains. The peculiar reddening of the pentabromide during sublimation, first observed by Balard, is in all probability owing to this cause. When the penta-compound fuses, it is partially decomposed into terbromide of phosphorus and free bromine, which colours the liquid red; the two rise in vapour together, thus producing a red gas; and when they reach a sufficiently cool portion of the vessel, recombine to form the lemon-yellow crystals of the penta-compound. It is not intended by this explanation to deny that pentabromide of phosphorus may be capable of volatilization without being decomposed. In a similar manner, the higher compound of iodine and phosphorus may be reduced by the aid of heat alone. The pentachloride, on the contrary, resists the decomposing action of heat.

The relative force of affinity between phosphorus and the three halogens to which my attention has been more especially directed, is in the order,—Chlorine, Bromine, Iodine. Thus, teriodide of phosphorus is decomposed by both chlorine and bromine; the terbromide by chlorine, but not by iodine; whilst the terchloride is not decomposed by either of the other elements. The same is indicated by the fact that terbromide of phosphorus distilled with chloride of mercury yields ter-

chloride of phosphorus, and bromide of mercury, which remains in the retort. None of these three ter-compounds is directly acted upon by oxygen gas; the terchloride of phosphorus is unaffected by sulphur at any temperature; the terbromide also appears to enter into no definite combination; yet, if the oxygen or sulphur be united with hydrogen, double decomposition ensues. Water, as is well known, resolves these ter-compounds into the hydracids and phosphorous acid: hydrosulphuric acid, as observed by Serullas, resolves terchloride of phosphorus into hydrochloric acid and tersulphide of phosphorus: a similar reaction takes place with the terbromide, which may be expressed by the formula—



When we consider the action of these non-metallic elements, and their hydracids, upon the penta-compounds, we are again reminded of the comparative feebleness with which the two additional atoms are combined. Pentachloride of phosphorus, it is true, is unaffected by bromine, and the action of iodine upon it did not appear to me well-defined; but pentabromide of phosphorus, carefully prepared so as to avoid excess of bromine, when brought into contact with iodine at the ordinary temperature, quickly ran down into a red liquid, and terbromide was found in the vessel. Very little iodine is required to effect this reaction; and since the compound IBr_5 is known to be readily formed, the decomposition in all probability may be thus expressed:—



The action of sulphur upon these penta-compounds is remarkable. If it be mixed with the pentachloride of phosphorus, and the whole fused together, combination ensues; but this is not a case of reduction; and the new products—a liquid and a crystalline body—require a more lengthy consideration than can be given them in this paper. Again, if pentabromide of phosphorus be mixed with sulphur, no action takes place in the cold; but upon gently raising the temperature, combination is instituted; the odour of bromide of sulphur is perceptible; and a liquid is produced, which appears to consist, partially at least, of a compound that will be presently described.

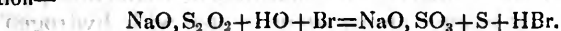
We have now to consider the manner in which these penta-compounds of the halogens with phosphorus frequently part with the two additional atoms when treated with hydrogen compounds. Hydriodic acid reduces the pentachloride of phosphorus to the terchloride, a fact first remarked by Se-

rullas*; yet I did not find the same occur with the pentabromide; nor was the pentachloride itself affected by a stream of hydrobromic acid†, even when heated. The proto-compounds of oxygen and sulphur with hydrogen act somewhat differently: the hydrogen enters into combination with the two atoms of the halogen that are more easily removable, and leaves the oxygen or sulphur in combination with the phosphorus compound. These substances in the chlorine series are the oxychloride of phosphorus described by Wurtz‡, having the composition PCl_3O_2 , and the sulphochloride of phosphorus discovered by Serullas§, bearing the formula PCl_3S_2 . In order to produce this oxygen compound, the action of the water must be moderated, or a secondary transformation into phosphoric and hydrochloric acids will take place. This may be effected either by throwing the white crystals of pentachloride of phosphorus into water, when they are instantly converted into the liquid oxychloride, and immediately afterwards drawing off the supernatant fluid; or, with greater ease, by exposing the solid chloride to the moisture of the atmosphere until the conversion is complete, hydrochloric acid being constantly evolved. A compound strictly analogous to this, may be obtained from the pentabromide of phosphorus by either of the two processes. The latter is of course preferable.

Oxybromide of Phosphorus.—When pentabromide of phosphorus is entirely decomposed by a moist atmosphere, there remains in the vessel a reddish liquid of a sticky consistency. This must be gently heated to expel hydrobromic acid, of which it contains a large quantity; and then, upon distillation at about 180°C. , a heavy vapour passes over, condensing into a colourless liquid of high specific gravity. It does not mix with water, but is slowly resolved by it into phosphoric acid and hydrobromic acid. It is soluble in oil of turpentine and in æther, and is also dissolved by strong sulphuric acid, from which it may be precipitated by water, unchanged, but soon suffering, of course, a secondary decomposition. Nitric acid destroys it violently with evolution of bromine. Bromine added to it reproduces momentarily the yellow crystals of

* *Ann. de Chim. et de Phys.*, xxxviii. 322.

† Hydriodic or hydrobromic acid in a gaseous condition may be conveniently prepared by the action of iodine or bromine on hyposulphite of soda moistened with a little water. The reaction will be evident from the equation—



In the case of iodine it is necessary to apply heat.

‡ *Ann. de Ch. et de Phys.*, 3rd ser. xx. 472.

§ *Ibid.* xlii. 25.

pentabromide; but the final result is a liquid, which, upon the application of heat, evolves bromine, and apparently leaves the original substance behind unaltered. Chlorine displaces the bromine in this compound, red vapours being immediately evolved when it is treated with that gas.

The analysis of this substance was effected by decomposing it in water, and precipitating the hydrobromic and phosphoric acids, thus produced, in the usual manner. The results, as given below, fully confirmed the expectation that this new liquid was the oxybromide of phosphorus, having the composition $\text{P Br}_3 \text{O}_2$.

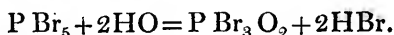
0.6245 grm. of substance yielded 1.227 grm. of bromide of silver, and 0.1514 grm. of phosphoric acid.

0.811 grm. of another preparation yielded 1.605 grm. of bromide of silver.

Or, calculated to 100 parts,—

	By experiment:		By calculation from $\text{P Br}_3 \text{O}_2$.
Phosphorus . .	10.77	...	11.29
Bromine . . .	82.50	83.11	83.06
Oxygen	5.65

The production of this oxybromide of phosphorus will be expressed by the equation—



I have not succeeded in accurately determining the boiling-point of the liquid under consideration. It appears to vary between 170°C ., and perhaps 30 degrees higher. Kopp finds that the substitution of three atoms of chlorine by three atoms of bromine in a compound, causes an elevation in its boiling-point of 96°C . Thus, the terchloride of phosphorus boils at 78°C .; the terbromide at 175°C . (Pierre), or 174°C ., according to my own observations. Now, as oxychloride of phosphorus boils at 110°C ., this would give as the boiling-point of oxybromide of phosphorus 206°C .; but experiment certainly does not indicate it as so high.

I may here remark that, in the decomposition of this oxybromide by water, there is usually formed a small quantity of a brown resinous-looking matter, not acted upon by water, nor soluble either in potash or nitric acid. It has a very characteristic odour. I have never obtained it in sufficient quantity for thorough examination. This readily accounts for the slight loss in the first analysis recorded above.

Sulpho-bromine Compound.—On passing a stream of hydro-sulphuric acid over pentabromide of phosphorus, combination instantly ensued; the yellow crystals gave place to a colour-

less or slightly red liquid, whilst hydrobromic acid was evolved: no free bromine or bromide of sulphur was observed. When the action was complete, the new product was first heated in a water-bath to drive off excess of hydrosulphuric acid, and then distilled at about 200° C. A colourless dense liquid passed over; but analysis showed that it was not the sulphobromide expected. The estimations, Nos. 1 and 2, recorded below, were made from it. Fearing that a decomposition might have taken place in the distillation of this substance, I submitted some carefully prepared pentabromide of phosphorus to the action of dry hydrosulphuric acid, and examined the resulting liquid itself. It was colourless, or slightly red, of high specific gravity, and fumed in moist air, giving forth a disgusting and deleterious odour. It was slowly decomposed by water, sulphur being deposited, and hydrobromic and phosphorous acids being dissolved: there was also in solution a sulphide of phosphorus, which gave a brown precipitate with a silver salt, but no hydrosulphuric acid. If alkali were added to the water, decomposition took place much more rapidly. It was soluble in æther, insoluble in cold sulphuric acid; but upon raising the temperature to about the boiling-point of the substance, decomposition ensued, the sulphuric acid became red, hydrobromic acid was evolved, and afterwards bromide of sulphur. Strong nitric acid attacked it violently; the phosphorus and sulphur being oxidized, the bromine dissipated. When treated with bromine, it gave momentarily the yellow crystals of pentabromide of phosphorus; then gas was evolved, and it again became liquid. It boiled at about 200° C. without alteration. The quantitative estimation of its elements gave analysis No. 3, proving its identity with the liquid first analysed.

As the hydrosulphuric acid employed in these experiments was made from sulphide of iron, and was dried by passing through sulphuric acid, I suspected that free hydrogen might possibly have interfered with the reaction, although it does not affect pentabromide of phosphorus itself when alone. The experiment was therefore repeated with hydrosulphuric acid prepared from sulphide of antimony, and dried by means of chloride of calcium. When the resulting colourless liquid was thoroughly saturated with the gas, it was found to be similar to that previously obtained. It afforded analysis No. 4.

The analyses numbered 1, 3, and 4 were performed by oxidizing the liquid by means of nitric acid. The second analysis was obtained by allowing the liquid to remain under water until entirely decomposed, precipitating the bromine by nitrate of silver, heating the precipitate with nitric acid in order

to destroy the brown salt produced at the same time, estimating the sulphuric acid thus formed in the usual manner, and adding the amount of sulphur it contained to that left undissolved in the aqueous solution.

I. 0.200 grm. of the liquid yielded 0.075 grm. of sulphate of baryta, and 0.0702 grm. of phosphoric acid.

II. 0.373 grm. yielded 0.07255 grm. of bromide of silver, 0.0125 grm. of sulphur, and 0.045 grm. of sulphate of baryta.

III. 0.4445 grm. yielded 0.173 grm. of sulphate of baryta, and 0.136 grm. of phosphoric acid.

IV. 0.2965 grm. yielded 0.124 grm. of sulphate of baryta, and 0.0848 grm. of phosphoric acid.

Or, reckoned to 100 parts,—

	I.	II.	III.	IV.
Phosphorus . .	15.6	...	13.6	12.8
Bromine	81.6		
Sulphur . . .	5.2	5.0	5.4	5.7

This does not accord well with any simple formula. If we suppose it to be a chemical combination of three atoms of terbromide with one atom of tersulphide of phosphorus, *i. e.* a sulpho-phosphite of the bromide of phosphorus, we obtain numbers which are similar.

	Calculated from $3\text{PBr}_3 + \text{PS}_3$.
Phosphorus	14.5
Bromine	80.1
Sulphur	5.4
	<hr/> 100.0

It is possibly, however, a mixture of two distinct substances having nearly the same boiling-point.

It does not appear that similar compounds belonging to the iodine series exist. Pentiodide of phosphorus is rapidly decomposed by the moisture of the atmosphere, but it did not furnish an oxyiodide. Hydrosulphuric acid has no action on the pentiodide of phosphorus, either at the ordinary temperature, or when fused by the heat of a water-bath.

It is remarkable to observe the increased stability which these two atoms of oxygen, or sulphur, give to the compound of phosphorus with three atoms of the halogen. Whereas hydrosulphuric acid is capable of completely decomposing both the terchloride and terbromide of phosphorus, it is incapable of removing the three atoms of chlorine from the sulphochloride of phosphorus, and also the remaining bromine from the compound of bromine, sulphur, and phosphorus just described. Again, whilst metals immersed in terchloride of phosphorus, or terbromide, are attacked by them, they remain

unaltered in oxychloride, or oxybromide of phosphorus, or the sulpho-bromine compound. Even potassium, which instantly inflames with violence when brought into contact with terchloride of phosphorus, will remain quite unaffected in the oxychloride, and combination is not determined even by boiling the liquid. It is true, the less easily oxidized metals, such as silver or platinum, are not attacked by terchloride of phosphorus at any temperature; but even platinum is acted upon by the pentachloride with the assistance of heat, the compound being entirely decomposed. And lastly, whereas the penta-compounds of the halogens with phosphorus may be reduced by means of additional phosphorus, no such reduction of the oxychloride takes place. This substance is also unaffected by iodine, bromine, or chlorine.

There appears no reason why a compound of phosphorus might not be formed, in which two of the halogens should be associated; but all my attempts to procure such a body have proved unavailing. Many of the experiments given above were undertaken with that view. It occurred to me also, that since chlorine decomposes both the pentabromide and iodide of phosphorus, hydrochloric acid might possibly effect a partial double decomposition; but I found it had no action upon either. If chloride of mercury and pentiodide of phosphorus be heated together, double decomposition does ensue; but it extends to the three atoms more intimately connected with the phosphorus, and the two additional atoms of iodine are set free: thus—

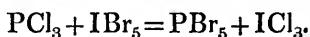


Since a halogen, bromine for instance, combines directly with the ter-compound of the same halogen with phosphorus, it appeared not improbable that it might combine readily with another ter-compound. But, when bromine is poured upon the terchloride of phosphorus, it merely sinks to the bottom, and the two liquids cannot be made to mix. If, however, a little iodine be added, a junction is instantly effected, attended with great evolution of heat: when the temperature falls, a red crystalline mass separates, resembling pentabromide of phosphorus when prepared with excess of bromine. In order to prove whether it was really this substance, or some new compound, it was necessary to separate it, if possible, from the accompanying liquid. This could not be effected by distillation, and the action of solvents was destructive: purely mechanical means therefore had to be resorted to. A compound of bromine and iodine was prepared, and terchloride of phosphorus was allowed to fall into it, drop by drop, as long

as the crystals appeared to increase in quantity; the supernatant liquid was then poured off, and the crystals were allowed to drain, without free exposure to the atmosphere, and were afterwards transferred to a porous tile also protected from moist air. When they appeared as dry as they were likely to become, they were analysed through decomposition by water, and precipitation of the hydracids by nitrate of silver.

0.431 grm. of the red crystals yielded 0.910 grm. of bromide of silver, and 0.113 grm. of chloride of silver.

This indicates 88.6 per cent. of bromine, and 6.4 per cent. of chlorine. Evidently, therefore, the crystalline body was no combination of both halogens with phosphorus, but merely pentabromide of phosphorus contaminated with a little of some chlorinated compound. The pentabromide requires 92.45 per cent. of bromine, and 7.55 per cent. of phosphorus. The reaction that takes place is in all probability very simple,—



If iodine be added to the terchloride of phosphorus, a red solution is immediately obtained. If this be subjected to a temperature a little below 100°C. , a red liquid distils over. Quantitative analysis showed that this was merely a solution of iodine in the terchloride; that element having passed over, in considerable quantity, with the vapour of the compound, at a temperature far below its own subliming point. In the same manner, if iodine be added to the terbromide of phosphorus, a red liquid is obtained, but without chemical combination.

Nor does it appear that direct combination will take place between phosphorus and the compounds of the halogens themselves. When the liquid chloride of iodine is added to phosphorus, violent combination ensues; iodine is liberated; and, even if a double compound be at all formed (which I doubt), it is destroyed by heat, and resolved into chloride of phosphorus and iodine. Again, if phosphorus be brought into contact with the orange-yellow crystals of terchloride of iodine, a blue colour is instantly developed, as though from the liberation of iodine, and a result is obtained similar to that in the former experiment.

Before quitting the compounds of bromine and phosphorus, I must mention a crystalline body, which I frequently obtained during my investigation of them, but which, from not understanding the conditions of its formation, I have never procured in any considerable quantity; and, in the attempt to purify it, I have almost uniformly destroyed the little I did possess. In distilling terbromide, or oxybromide of phosphorus, there remained in the retort, on several occasions, a

small amount of these crystals; and they were observed to form on the yellow pentabromide of phosphorus, when imperfectly secured from the action of the atmosphere, but before complete conversion into oxybromide. They are perfectly transparent and colourless, with sharply-defined edges; they are decomposed by water: by the moderated heat of a spirit-lamp they fuse, and immediately sublime. But this fusion is apt to produce an alteration in them: instead of re-crystallizing on cooling, a permanent liquid is sometimes found, having the characters of oxybromide of phosphorus. The only analysis I obtained was made with a very small quantity.

0.1125 grm. yielded 0.2255 grm. of bromide of silver, and 0.048 of phosphate of magnesia.

Or, reckoned to 100 parts,—

Phosphorus	12.0
Bromine	84.2
Oxygen, or loss	3.8
	<hr/>
	100.0

Is this crystalline body isomeric with liquid oxybromide of phosphorus? or is it some compound containing less oxygen? May it not also possibly account for the variations in the boiling-point of the oxybromide?

We are unacquainted, I believe, with any method by which the iodides of phosphorus can be purified from excess of iodine. Thus they have never been analysed. It is on this account, also, that my researches have not been carried so far with the iodine as with the bromine compounds; nor can I feel perfect confidence in all the results at which I have arrived in reference to them.

Thus have we seen the manner in which the halogens combine with phosphorus, forming ter- and penta-compounds; and also some methods by which the compounds containing five atoms of the halogen may be reduced to those containing three. The comparative feebleness of the combination of the two additional atoms has been rendered still more evident by the action of certain elementary bodies, and of the compounds of these elements with hydrogen. Among the products of these reactions are an oxybromide of phosphorus, and a sulphur compound of less simple formula. It has also been remarked how the substitution of oxygen, or sulphur, for two of the elements of halogen in a penta-compound, increases the force with which the remaining three are combined. And lastly, the great indisposition of any two halogens to enter together into a phosphorus compound has likewise been observed.

XLV. On Shooting Stars.

By Sir J. W. LUBBOCK, Bart., F.R.S.*

SOME time since, in a paper in the Philosophical Magazine, I ventured to suggest that the disappearance of shooting stars might be occasioned by their passing into the earth's shadow, or in other words, by being eclipsed. Lately, in looking over the *Astronomische Nachrichten*, I found in No. 614, in a paper entitled "Ueber eine Längenbestimmung aus den August-Sternschuppen," 1847, by M. Weyer, three observations of shooting stars, which might, supposing their distance from the earth's surface as given in that paper sufficiently accurate, serve to test in those instances the truth of my hypothesis. Accordingly I requested Mr. Farley to calculate the distance of the bodies, and the following tables exhibit the results obtained, and the data, as furnished by the tables in the *Ast. Nach.* They do not appear to be favourable to the hypothesis above referred to. The notation is as follows, conformably to that of my preceding paper:—

R = earth's semidiameter = 3958 miles.

α = azimuth of shooting star — azimuth of sun; reckoned from the south meridian westward.

ζ = apparent zenith distance of shooting star.

ρ = distance of shooting star from the spectator.

θ = depression of sun's centre, so that $90^\circ + \theta$ = sun's zenith distance.

1847.	Mean time at Hamburg.	Place of disap- pearance.	Mean time at Pa- penburg.	Place of dis- appearance.	Distance from the earth's surface in	
					German miles.	English miles.
Aug. 10..	^h ^m ^s 10 54 2	Herculis	^h ^m ^s 10 43 53	α Pegasi ...	12	55
	10 58 43	Lyræ ...	10 48 34	α Androm.	18	83
	11 40 25	Lyræ ...	11 30 16	β Arietis...	10	46

In order to estimate the effect which an error in the assumed place of disappearance would have upon g , it has been calculated upon three different hypotheses:—

1. Supposing the shooting star to have disappeared at Papenburg, having the same R.A. and Dec. as the star indicated in the *Ast. Nach.*

2. Supposing the place so assumed to be in error 3° or 12^m in R.A., and the Dec. to be correct.

3. Supposing the place assumed to be in error 3° in Dec. and the R.A. to be correct.

* Communicated by the Author.

The last column contains the value of ρ as obtained from the distance from the earth's surface given in the *Ast. Nach.* For the purpose of comparison, it will be seen that much greater errors than those I have assumed in the place of the shooting star's disappearance would be required to conciliate the two results.

We found for the instant of disappearance and for Papenburg,—

	Assumed place of disappearance.		ζ .	Azimuth.	δ .	\odot 's Azimuth.	α .	ρ in English miles.	
	R.A.	Dec.							
1....	h m								
1....	22 57	+14 23	52 9	300 44	19 9	159 16	141 28	555	84
2....	23 9	+14 23	53 43	297 40	19 9	159 16	138 24	591	84
3....	22 57	+17 23	49 38	298 36	19 9	159 16	139 20	500	84
1....	0 1	+28 15	49 32	275 44	19 21	160 12	115 32	422	124
2....	0 13	+28 15	51 20	273 52	19 21	160 12	113 40	436	124
3....	0 1	+31 15	47 22	272 57	19 21	160 12	112 45	393	124
1....	1 46	+20 4	65 9	269 16	20 55	171 8	98 8	633	170
2....	1 58	+20 4	66 57	266 53	20 55	171 8	95 45	649	170
3....	1 46	+23 4	62 21	267 6	20 55	171 8	95 58	579	170

It seems very desirable to procure other observations of this kind made by two or more persons, having their time sufficiently accurate to be able to identify the meteor, and by each noting as accurately as possible the place of disappearance, so that they may be enabled through the parallax of the body to determine its distance from the earth. I do not see in what manner isolated observations by one person can in the present state of the subject tend to elucidate this interesting question. The paper in the *Ast. Nach.*, to which I have referred, affords ample evidence that such combined efforts would in all probability be attended with success. The determination of the distance of the body by means of its parallax is of course independent of any hypothesis as to its nature, and may therefore serve as a guide in any reasoning upon that point.

XLVI. *Remarks on the Weather during the Quarter ending September 30, 1849.* By JAMES GLAISHER, Esq., F.R.S., F.R.A.S., and of the Royal Observatory, Greenwich*.

A MORE than ordinary interest is attached to the meteorology of the present year, and in particular to that part of the year just passed, owing to the presence of cholera as an

* Communicated by the Author.

epidemic complaint. This disease has been fatal during the last quarter to a great number of persons. The number of deaths in London and in its vicinity, according to the weekly Reports of the Registrar-General, exceeded the average number of deaths in July by 2515, in August by 4464, and in September by 4322, out of a population of 2,206,076; a mortality unprecedentedly great.

I have no means of judging of the amount of illness, but it must have been great.

If epidemics be due to atmospheric causes acting upon local circumstances, it is to the successful cultivation of medical meteorology that we can hope to contend with them with success.

Those causes of sickness which depend upon temperature, humidity, pressure or movement of the air, admit of measurement; and it must be considered as a most valuable circumstance, that nearly forty educated gentlemen—known and trustworthy observers, with good instruments—were daily engaged, long before the epidemic came, in collecting this information. Without this organization our knowledge of the meteorological particulars of the period would have been as deficient as it is of the year 1832, when the cholera was before prevalent.

The results thus collected may now be compared with those of former years, as far as the previous years will furnish the materials of comparison, with those for instance when epidemics prevailed, as in 1832, and with those of non-epidemic years, as in 1842, a year which was unusually free from epidemic diseases. Those gentlemen who may wish to investigate the connexion which may have existed between the sickness and mortality of the seasons, with their meteorological particulars, will find the monthly values of the subjects of meteorological research in the Quarterly Reports of the Registrar-General, with the names of the gentlemen who have furnished the observations; and most of those gentlemen would be happy to furnish detailed copies from their registers for short periods to any gentleman who would use them in connexion with sickness or mortality.

The quarterly meteorological returns for the past quarter, furnished to the Registrar-General and myself, have been received from thirty-eight different places, whose returns have been found on examination to be good. These have all been examined and reduced by myself.

At Greenwich the mean daily temperatures of the air from July 1 to July 17 were above their average values; the mean excess was $3^{\circ}\cdot 2$. From July 18 to August 5 they were below

their average values; the mean deficiency was $2^{\circ}2$. From August 6 to August 12 the temperature was high; its mean daily excess was $6^{\circ}0$. From August 13 to August 19 the mean deficiency was $1^{\circ}9$. From August 20 to September 15 the temperature was high; its mean excess was 4° ; and this period was distinguished by a thick and stagnant atmosphere, and the air was for the most part very close and oppressive. The temperature was $3^{\circ}3$ below its average from September 11 to September 21; and it was $5^{\circ}5$ in excess from September 22 to the end of the quarter. The summer has been warm and dry, without great heat. Thunder-storms have been very frequent during the quarter. The air has been for the most part unusually dry. The amount of rain has been less than usual in latitudes south of 53° , and north of this parallel it has been greater. The magnets have been seldom disturbed during the quarter; and the amount of electricity, though less than usual, seems to have been so in consequence of the less amount of humidity of the air.

The weather has been much finer in the southern than in the northern parts of the country.

The mean temperature of the three months ending August, constituting the three summer months, was $61^{\circ}0$; and that of the average of seventy summers is $60^{\circ}0$.

The several subjects of research in the past quarter are as follows:—

*The mean temperature of the air** for the month of July was $62^{\circ}1$, which is $4^{\circ}3$, $1^{\circ}9$, $1^{\circ}2$, $0^{\circ}7$, $2^{\circ}3$ and $0^{\circ}6$ above those of the years 1841, 1842, 1843, 1844, 1845 and 1848 respectively; and is $2^{\circ}4$ and $3^{\circ}3$ below those of the years 1846 and 1847 respectively. The temperature of this month exceeded its average from seventy years by $0^{\circ}7$.

For the month of August was $62^{\circ}9$, which is $2^{\circ}4$, $0^{\circ}8$, $5^{\circ}2$, $5^{\circ}6$, $0^{\circ}8$ and $4^{\circ}4$ above those of the years 1841, 1843, 1844, 1845, 1847 and 1848 respectively; and is $2^{\circ}5$ and $0^{\circ}3$ below those of the years 1842 and 1846 respectively. The temperature of this month exceeded its average from seventy years by $2^{\circ}1$.

For the month of September was $58^{\circ}8$, which is $0^{\circ}7$, $2^{\circ}4$, $1^{\circ}9$, $5^{\circ}2$, $4^{\circ}5$ and $3^{\circ}0$ above those of the same month in the years 1841, 1842, 1844, 1845, 1847 and 1848 respectively; and is $0^{\circ}7$ and $1^{\circ}3$ below that in 1843 and 1846. The temperature of this month exceeded that of the same month on an average of seventy years by $2^{\circ}1$.

* For the monthly, quarterly and yearly mean temperature of the air from 1786 to 1843, see my paper in the Philosophical Transactions, part ii. 1849; and for the mean monthly values of several meteorological elements, see my paper on Meteorology in the Illustrated London Almanac for 1850.

The mean temperature of evaporation at Greenwich—

For the month of July was $56^{\circ}2$; for August was $57^{\circ}3$; and for September was $54^{\circ}6$. These values are $1^{\circ}3$ less, $0^{\circ}4$ less and $0^{\circ}1$ greater, respectively, than the averages of the same months in the preceding eight years. The mean value for the quarter was $56^{\circ}0$, which is $0^{\circ}5$ below the average of the corresponding quarters of the eight preceding years.

The mean temperature of the dew-point at Greenwich—

For the months of July, August and September, were $51^{\circ}1$, $53^{\circ}0$ and $51^{\circ}0$ respectively. These values are $3^{\circ}6$, $2^{\circ}4$ and $1^{\circ}6$ below the average of the same months in the preceding eight years. The mean value for the quarter was $51^{\circ}7$, being of less value than in the same quarter of each of the eight preceding years, and less than their average by $2^{\circ}5$. This implies that less water has been mixed with the air during this period than in any of the corresponding quarters for eight years.

The mean elastic force of vapour at Greenwich for the quarter balanced a column of mercury 0.400 inch in height. Its average value for the corresponding quarter of the eight preceding years was 0.433 inch. The difference implies great dryness of the air.

The mean weight of vapour in a cubic foot of air for the quarter was 4.5 grains. The average from the eight preceding years was 4.9 grains.

The mean additional weight of water required to saturate a cubic foot of air was 1.6 grain. This value, from the eight preceding years, was 1.1 grain.

The mean degree of humidity in July was 0.711 , in August was 0.772 , and in September was 0.772 . The averages for the eight preceding years were 0.798 , 0.837 , and 0.858 respectively.

The mean reading of the barometer at Greenwich in July was 29.789 inches, in August was 29.841 , and in September was 29.767 . These values are respectively 0.011 less, 0.060 greater, and 0.048 less than the averages of the same months for the preceding eight years.

The reading of the barometer was 30.03 inches at the beginning of July. It decreased to 29.56 on the evening of the 4th, when it began to increase, and passed the point 30 inches during the afternoon hours of the 8th, and was 30.25 on the 11th at 9 A.M. This was the highest reading in the month. The reading decreased below 30 inches on the 15th, and to 29.40 by the morning of the 20th; it then increased to 29.94 by noon on the 23rd; it then decreased to 29.31 on the 25th, and this was the lowest reading in the month; it then increased to 29.80 by the evening of the 28th; then decreased to 29.52 at 3 P.M. on the 30th; and after this increased to 29.80 at

the end of the month. The range of readings during the month was 0·94 inch.

In August the reading increased to 30·02 inches on the morning of the 2nd at 9 A.M.; decreased to 29·78 on the 5th at 10 A.M.; increased to 29·95 on the 7th at 9 A.M.; decreased to 29·56 by the 9th at 9 P.M.; increased to 29·95 on the 7th at 9 A.M.; decreased to 29·56 by the 9th at 9 P.M.; increased to 29·79 on the 11th at 9 A.M.; decreased to 29·46 by the 13th at 3 P.M.; increased to 29·74 by 3 P.M. on the 15th; decreased to 29·54 on the 16th at 3 P.M.; it then increased and passed the point 30 inches about midnight on the 18th, and was 30·22 on the 21st; at which reading it continued almost all the day, and then decreased, slowly passing below 30 inches after noon on the 25th, to 29·69 on the 30th at 9 P.M., and then increased to 29·74 by the 31st. The range of readings in the month was 0·66 inch.

The reading decreased on September 1st to 29·50 inches at 9 P.M.; then increased slowly till the 7th at midnight, when it was 30·05; the decrease on the 8th was slow; it was rapid on the 9th and 10th; and the reading on the 11th was 29 inches nearly all the day; on the 12th at noon the reading was 28·88, being the lowest in the quarter; after this time the reading increased, and passed the point 30 inches before noon on the 14th; and continued to increase till the 19th at noon, when it was 30·36, being the highest in the quarter; after this time it decreased, and passed below 30 inches before noon on the 22nd, and continued to decrease till the 27th at 3 P.M., when it was 29·61; it then decreased slightly to 29·73 of the morning of the 28th; and then decreased to 29·08 by the end of the month. The range in the month was 1·48 inch, being greater than usual.

The average weight of a cubic foot of air, under the average temperature, humidity and pressure, was 524·6 grains. The average for the preceding eight years was 525·8 grains.

The rain fallen at Greenwich in July was 2·9 inches, in August was 0·45, and in September was 3·3. The amount for the quarter was 6·6. The average for this quarter in the preceding eight years was 7·2.

The fall of rain in August was less than has fallen in August since the year 1819. The average fall of rain at Greenwich, from thirty-three years' observations, in July is 2·5, in August 2·4, and in September 2·4 inches. The fall was less than its average at places south of latitude 53°, exclusive of Cornwall and Devonshire; it was about its average fall between 53° and 54° of latitude; and north of 54° the fall was greater than usual.

The excess of rain in the quarter in the counties of Cornwall and Devonshire is owing to two remarkable falls which occurred in Cornwall on September 22 and on September 26. The observer at Helston mentions the falls on the nights of those days as very remarkable. The observer at Falmouth says, "a greater quantity of rain fell on the nights of the 22nd and 26th of September than I have measured in the same time for twelve years, viz. 1·925 and 1·964 inches respectively." The observer at Truro says, "the quantity of rain for September is most extraordinary, amounting to 9·25 inches, particularly the amount which fell on the 22nd, viz. 4·24 inches. On the 26th a large quantity also fell, viz. 3·00 inches. The total for the month exceeded that registered in any previous month during the period of our observations (eleven years), except in one instance, which, in the same month of September, slightly exceeds the present amount. The largest fall in any one day previously noted was 2·10 inches (in December 1848); so that the quantity on the 22nd of September is double that on any former occasion, and is rendered more remarkable by being followed in four days by another far exceeding all except itself." These remarkable falls seem to have been confined almost entirely to the county of Cornwall.

The directions of the wind at Greenwich were S.W. and N.W. till July 8; was N.E. from the 10th to the 16th; and it was mostly S.W. from the 17th to the end of the month. From August 2 to 6 was N.W. and N.E.; it was S.W. from the 8th to the 17th; and was chiefly N.W. till the end of August. It was mostly N. from September 1 to the 8th; S.W. from the 10th to the 16th; and N. and N.E. after this time. From the observations of the direction of the wind which have been taken daily at most of the principal railway stations, and published in the "Daily News" on the following day during the whole of the past quarter, it appears that the general direction was the same all over the country when the air has been in quick motion; but that at other times its direction has been variable, and very frequently in a calm state at places whose elevation is inconsiderable.

The daily horizontal movement of the air in July was 120 miles; from August 1 to 11th was 50 miles; from August 12 to 16th was 170 miles; and from August 17 to the end of the quarter was about 55 miles, except on September 11 and 12th, when it amounted to 190 miles daily. The average daily horizontal movement of the air during the quarter is about 120 miles. Therefore during the months of August and September the movement of the air was about one-half the usual amount.

This remark applies to the place at which Osler's anemometer is placed at Greenwich, viz. at an elevation of upwards of 200 feet above the level of the sea, and near the northern extremity of the table-land forming Blackheath. At places at a less elevation, the movement of the air was very much less than the above; on many days, when a strong breeze was blowing on the top of the observatory and over Blackheath, there was not the slightest motion in the air near the banks of the Thames; and this remarkable calm continued for some days together, particularly from August 19 to the 24th, on the 29th, from September 1 to the 10th, and after September 15. On September 11 and 12 the whole mass of air at all places was in motion, and the first time for nearly three weeks, the hills at Hampstead and Highgate were seen distinctly from Greenwich.

From the published observations of the strength of the wind daily at all parts of the country, it would seem that the air has been for days together in a stagnant state at all places whose elevation above the sea is small.

The readings of the thermometer on grass in July were below 40° on four nights; the lowest was $32^{\circ}8$; between 40° and 50° on twenty nights, and above 50° on seven nights. In August the readings were below 40° on five nights; the lowest reading was $34^{\circ}5$; between 40° and 50° on ten nights, and between 50° and 60° on twelve nights. In September the readings were below 40° on six nights; between 30° and 40° on twelve nights, and about 50° on ten nights.

At Cardington the lowest reading on grass in July was $31^{\circ}8$, in August was 32° , and in September was 27° . The mean of all the lowest readings was $45^{\circ}5$ in July, $47^{\circ}6$ in August, and was $44^{\circ}1$ in September, as observed by Samuel Charles Whitbread, Esq.

There were three exhibitions of the *aurora borealis*. The first was seen on August 18 at Whitehaven; the second was seen at Latimer on September 3 at 8 P.M., when a rose-coloured auroral arch was seen extending from south-west to north-east across the zenith; and on Sept. 16 an aurora was seen at Stonyhurst.

Thunder-storms occurred on July 18 at Nottingham and Leicester; on July 19 at Camberwell, Saffron Walden, Uckfield and Greenwich; on July 20 at Nottingham, Camberwell, Saffron Walden, Leicester, Uckfield and Greenwich; on July 23 at Hartwell Rectory, Stone and Leicester; on July 24 at Camberwell; on July 25 at Hartwell Rectory, Stone and Camberwell; on July 26 at Camberwell, Leicester and Greenwich; on July 29 at Nottingham and Leicester; on

August 1, 2 and 3, at Nottingham; on August 4 and 6 at Helston; on August 7 at Helston and Leicester; on August 8 at Hartwell Rectory, Stone, and severe at Uckfield; on August 9 at Nottingham, Cardington, Liverpool and Leicester: that at Nottingham is described as being violent. On August 10 at Liverpool and Leicester; on August 11 at Cardington, Hartwell Rectory, Stone and Uckfield; on August 12 at Nottingham, Saffron Walden and Leicester; on August 13 at Saffron Walden; on August 17 at Leicester and Uckfield; on August 30 at Nottingham; on September 1 at Stone, Uckfield very severe, and Leicester; on September 2 at Hartwell Rectory, Uckfield and Saffron Walden: at this place the storm was very violent. On September 3 at Stone, Uckfield and Leicester; and at Uckfield on September 4 and 10.

Lightning was seen but thunder was not heard on July 19 and 20 at Saffron Walden; on July 20 and 23 at Hartwell Rectory and Stone; on August 4 and 6 at Helston; on August 7 at Helston, Uckfield and Greenwich; on August 8 at Hartwell Rectory and Stone; on August 9 at Cardington, Liverpool and Stonyhurst; on August 10 at Liverpool; on August 11 at Cardington, Hartwell Rectory, Stone, Southampton and Greenwich; on August 12 at Uckfield, Southampton and Saffron Walden; on August 13 at Saffron Walden; on August 17 at Cardington; on August 18 at Cardington and Greenwich; on August 19 at Cardington; on August 20 at Greenwich; and on August 24 at Uckfield.

Thunder was heard but lightning was not seen on July 18 at Cardington and Holkham; on July 19 at Helston, Cardington, Hartwell and Stone; on July 20 at Hartwell Rectory and Stone; on July 22 at Helston; on July 23 at Helston, Cardington and Holkham; on July 25 very heavy thunder was heard at Latimer; on July 26 at Cardington, Hartwell, Stone, Southampton and Nottingham; on July 27 at Latimer; on July 31 at Cardington and Newcastle; on August 8 at Holkham, Norwich and Newcastle; on August 9 at Holkham, Latimer, Newcastle and Stonyhurst; on August 10 at Newcastle; on August 11 at Latimer and Newcastle; on August 12 at Holkham; on August 13 at Cardington, Hartwell and Stone; on September 1 at Holkham, Hartwell Rectory, Latimer and Southampton; on September 2 at Helston and Latimer; on September 3 at Holkham and Hartwell; on September 5 at Holkham and Wakefield; on September 6 at Wakefield; on September 10 at Wakefield and Liverpool; on September 20 at Helston; on September 28 at Newcastle; and on September 30 at Nottingham.

Hail fell on August 8 at Uckfield: the observer mentions that the hailstones were as large as beans. Hail also fell on August 12 at Saffron Walden.

Solar halos were seen at the following places:—On July 1 at Maidenstone Hill, Stone and Nottingham; on July 2 at Stone; on July 3, 19 and 21 at Maidenstone Hill; on July 25 and August 1 at Stone; on August 9 at Hartwell and Stone; on September 1 at Maidenstone Hill; on September 22 at Stone; and on September 26 and 28 at Maidenstone Hill.

Lunar halos were seen at Cardington on July 5, and at Maidenstone Hill on September 2 and 28.

I have been favoured with the following agricultural reports. At Guernsey, by Dr. Hoskins, F.R.S.

The weather during July was uniformly fine; the quantity of rain rather above the average, distributed in equable showers from the 18th to the end of the month. There was less thunder and lightning than usual.

The crops without exception luxuriant. The mean temperature of August was high, which, added to rain much below the usual average, enabled the farmers to secure the harvest speedily and profitably. The earlier half of September was warm and dry, the latter wet and windy. Potatoes small, but good and abundant; wall-fruit scanty, but figs in large quantities, and thoroughly ripened. Cider apples scanty.

About the beginning of August cholera appeared in ill-drained districts as an epidemic; it spread erratically in almost every part of the town and suburbs, and afterwards appeared in isolated country houses, in which no morbid cause could be traced. It declined towards the end of September.

Small-pox was also very general during this and the previous quarter, as well in the country-places as in town. Vaccination had been much neglected, owing to indifference and prejudice on the part of the lower orders.

At Uckfield, by C. L. Prince, Esq., Surgeon.

The weather during the months of July, August and September, has been very fine, warm, dry, and remarkably healthy, the mortality having been lower during this quarter than in the corresponding quarter for several years past. The temperature has been upon the whole very equable, and without that *excessive* heat which usually characterizes a warm and dry summer in the southern counties. The crops of hay and of every species of grain has been abundant, very good in quality, and secured in excellent condition. The hop plant has been much diseased, and the crop far below the average.

The failure of this crop is a great loss to the poor in this district; as from their earnings in hop-picking they are generally enabled to buy a certain amount of clothing, as well as sundry other necessaries for the winter. The potatoe haulm has been diseased in some situations, but I do not find that the tuber has been in any way injured. The crop of apples is good, and above the average; but that of the pear, plum, and wall-fruit generally, is almost entirely deficient, the blossom and trees having been much injured by the heavy snow which fell in April.

At Stonyhurst, by the Rev. Alfred Weld, F.R.A.S.

Potatoes were first got up about June 30; it was then found that about 1 lb. out of 20 lbs. was diseased: still there were no signs of decay in the leaves, which looked strong and healthy. About August 20 the tops of the potatoes seemed struck by a general blight, which spread with such rapidity that in two or three days from that period the fields were quite black. The roots suffered at the same time in a greater or less degree; frequently the proportion of decayed to sound potatoes was as two to one. Potatoes planted on damp soil always suffered most, while others planted in sheltered spots escaped with comparative immunity. It was found that in one case, where clay and black bog soil existed in the same field, potatoes planted on the former suffered severely, while those on the latter remained untouched. The smell of the decaying tops was offensive, and so strong as to be perceptible at a considerable distance. After July 15 the weather became very unfavourable for hay, and a great deal remained out until August. The crop was far below the average. A distemper broke out amongst horses about the middle of July, and was followed by another which attacked the cattle; both were in some instances fatal. Reaping of wheat began August 25th; of oats, August 31st. The crops were far above the average, and generally well-housed. The average length of oat-straw grown on a field of six acres, which had not been ploughed before for more than twenty years, was six feet, whilst in some places it was above seven feet. The grain was with few exceptions all housed by the end of September.

For the West Riding of Yorkshire, by Charles Charnock, Esq., of Leeds.

The past quarter has consisted of one wet and of two dry months. The growth of turnips and potatoes were retarded during the dry months, and progressed rapidly during that which was wet. The harvest was very protracted on the early soils, but secured in very good condition. On late soils corn is still exposed to the weather. On the 2nd of October

I saw corn, both reaped and unreaped, covered with snow in some districts.

The potatoe crop is not heavy, but on the whole nearly free from the disease which has been so fatal for several years. Much alarm was caused by the tops of many fields being discoloured by the frost about the middle of September; but on examination, the tubers are found to be not much affected.

Wheat is a bulky crop, but does not yield well. Barley and oats are much below an average bulk.

Live stock is generally healthy, except cattle imported from Ireland, which are mostly affected with diseases of the lungs.

The heavy rains which fell on the 28th and 29th of September will no doubt be of much use in many ways. The river Aire, which passes through Leeds, was much swollen, and its waters gave evident proof of some of the causes of cholera. At Castleford, whose distance from Leeds is ten miles, their stench was greater than can be imagined; whilst their deleterious contents were such that all the fish were almost destroyed or taken in a stupefied state, and large quantities floated upon the surface of the water.

At Finsbury Farm, near Romsey, by J. Clark, Esq.

The harvest was well saved, and generally an average crop. The season has been, and is still, all that could be desired. Grass and turnips are growing beautifully, and agricultural operations are proceeding satisfactorily. Some wheat has been sown on heavy lands in fine order. The early tares, rye, and clover, are doing well.

To the Report of the Registrar-General are appended the monthly values at every station, from which the average values for the quarter have been determined, and which are contained in the following table:—

The mean of the numbers in the first column is $29\cdot576$ inches, and this value may be considered as the pressure of dry air for England during the quarter ending September 30, 1849.

The mean of the numbers in the second column, for Guernsey and those places situated in the counties of Cornwall and Devonshire, is $59^{\circ}\cdot5$; for those places situated south of latitude of 52° , including Chichester and Hartwell, is $60^{\circ}\cdot1$; for those places situated between the latitudes of 52° and 53° , including Saffron Walden and Leicester, is $58^{\circ}\cdot4$; for those places situated between the latitudes of 53° and 54° , including Derby and York, is $57^{\circ}\cdot1$; at Liverpool and Whitehaven is $57^{\circ}\cdot7$; and at Durham and Newcastle is $55^{\circ}\cdot8$.

The average daily range of temperature in Cornwall and Devonshire was $12^{\circ}\cdot9$; south of latitude 52° was $19^{\circ}\cdot3$; between the latitudes of 52° and 53° was $19^{\circ}\cdot3$; between the

Meteorological Table for the Quarter ending September 30, 1849.

Names of the places.	Mean pressure of dry air reduced to the level of the sea.	Mean temperature of the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Mean monthly range.	Range of temperature in the quarter.	Mean temperature of the dew-point.	Wind.		Mean amount of cloud.	Rain.		Mean weight of vapour in a cubic foot of air.	Mean additional weight of vapour required to saturate a cubic foot of air.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Height of cistern of the barometer above the level of the sea.
									Mean estimated strength.	General direction.		Number of days on which it fell.	Amount collected.						
Guernsey	29.600	61.0	79.5	53.0	10.0	21.3	26.5	52.8	1.6	w. by n.	..	31	in. 5.2	grs. 4.6	grs. 1.3	0.783	5.7	527	123
Helston	29.547	59.7	80.0	41.0	14.0	30.3	39.0	56.2	1.4	Variable.	6.9	38	10.5	5.2	0.7	0.879	6.4	527	106
Falmouth	59.4	78.0	40.0	16.5	34.0	38.0	..	0.8	w. & n.e.	6.3	41	10.7	4.7
Truro	29.643	58.3	72.0	43.0	10.2	23.0	29.0	52.6	1.9	w. & e.	..	38	16.5	4.6	1.4	0.773	5.8	528	140
Torquay	29.656	60.2	75.0	46.0	9.9	24.0	29.0	53.5	1.9	w. & e.	3.7	39	8.7	4.6	1.2	0.795
Exeter	29.656	58.4	79.5	36.0	17.1	34.3	43.5	53.5	..	Variable.	..	48	7.0	4.7
Chichester	29.695	59.5	78.0	41.0	13.9	31.7	37.0	w. & e.	5.5	..	7.1	4.6	1.3	0.792
Uckfield	29.705	59.7	84.0	36.0	21.6	43.3	48.0	53.1	..	Variable.	6.9	41	6.6	4.9	1.3	0.792
Southampton	29.760	59.7	83.2	38.0	16.1	38.4	45.2	53.2	0.2	w. & e.	5.5	27	8.1	4.6	1.3	0.792
Beckington	29.766	58.6	82.0	29.0	21.8	43.3	53.0	50.1	1.0	s.w. & n.w.	4.2	30	3.8	4.4	1.6	0.782	5.8	528	180
Royal Observatory, Greenwich.	29.588	61.3	84.1	39.5	20.1	40.4	44.6	51.8	..	s.w. & n.w.	6.4	30	6.7	4.5	1.6	0.752	5.5	525	265
Maidenstone Hill, Greenwich.	29.586	60.2	84.1	42.9	15.8	35.2	41.2	53.4	..	s.w.	..	33	6.5	4.7	1.3	0.786	5.8	526	107
Chiswell Street, London	29.568	62.6	87.0	40.0	11.1	30.8	38.0	53.6	..	s.w.	4.7	1.7	0.736	6.0	524	200
St. John's Wood	29.587	60.9	80.2	41.0	20.4	41.1	45.2	54.2	1.6	Variable.	6.9	41	6.6	4.9	1.3	0.792
Camberwell	60.5	85.0	43.0	15.0	43.3	42.0	54.5	2.6	s.w.	7.4	36	6.0	4.3	2.1	0.680	5.3	519	335
Latimer Rectory	29.600	61.3	85.0	36.0	24.9	43.3	59.0	51.0	1.6	s.w. & n.e.	6.3	42	8.3	4.3	1.6	0.726	5.3	523	280
Aylebury	29.571	60.3	83.0	36.0	21.4	43.0	47.0	50.8	0.7	s.w. & s.e.	6.3	37	8.8	4.3	1.6	0.726	5.3	523	280
Stone Observatory	29.571	58.7	81.6	36.4	19.6	42.4	45.2	49.7	0.9	s.w. & s.e.	5.6	46	6.5	4.2	1.5	0.739	5.1	524	320
Hartwell (near Aylesbury)	29.545	58.7	85.0	38.0	28.5	42.8	47.0	53.4	1.2	s.w. & n.e.	6.0	32	8.1	4.7	1.1	0.705	5.8	524	260
Hartwell Rectory	29.610	58.7	81.0	38.0	17.3	37.3	43.0	50.7	0.9	n.w. & n.e.	3.9	45	..	4.3	1.4	0.756	5.3	526	260
Saffron Walden	29.586	58.6	84.0	40.0	16.0	38.3	40.0	..	2.0	s.w. & n.e.	4.0	36	6.5	250
Radcliffe Observatory, Oxford.	29.641	59.2	52.2	1.9	s.w. & n.e.	7.2	29	6.4
Hereford	56.3	59.0	83.4	37.3	17.3	40.9	46.1	52.3	..	Variable.	..	35	7.8
Cardington, near Bedford.	29.630	59.0	83.4	40.0	16.3	35.7	41.0	53.4	..	w. & n.e.	6.4	36	8.2	4.5	1.4	0.765	6.6	527	70
Norwich	29.508	58.7	81.0	40.0	16.3	35.7	41.0	53.4	..	s.w.	7.3	31	5.1	4.7	0.9	0.841	5.8	520	39
Holkham	29.563	59.4	84.4	39.0	15.0	39.0	45.4	50.6	0.8	s.w. & n.e.	6.7	39	4.7	4.3	1.7	0.717	5.3	528	31
Leicester	29.080	57.5	86.0	33.0	21.9	47.3	53.0	50.2	1.3	s.w. & n.e.	5.5	42	9.8	4.2	1.3	0.777	6.2	529	156
Derby	29.434	57.6	80.0	35.0	13.0	37.0	45.0	51.7	..	s.w. & n.e.	4.5	45	7.2	4.5	1.2	0.802	5.8	528	39
Highfield House, Notts.	29.509	58.4	85.0	33.8	21.7	43.8	51.2	53.6	0.8	Variable.	7.1	50	9.0	3.8	0.8	0.849	5.8	527	103
Liverpool Observatory	29.589	59.1	77.6	46.0	10.1	26.0	31.6	49.7	1.2	s.w. & n.e.	6.3	45	10.8	4.2	0.9	0.812	5.3	533	37
Wakefield	29.574	57.9	91.0	33.0	21.8	47.0	58.0	50.2	..	w. & n.e.	..	46	4.2	4.2	1.3	0.762	5.1	528	113
Stourton Lodge (near Leeds)	29.543	57.2	82.0	39.0	15.8	36.0	53.0	53.9	1.5	n.w. & n.e.	5.5	45	8.4	4.8	0.7	0.880	6.9	528	148
Stonyhurst Observatory	29.589	55.3	80.5	34.3	15.3	37.2	46.2	50.6	1.2	Variable.	7.1	60	18.0	4.4	0.8	0.865	5.3	526	381
York	29.535	56.0	82.0	38.0	14.3	34.3	44.0	53.4	..	Variable.	..	37	8.3	4.7	0.8	0.866	5.8	529	50
Durham	29.506	56.2	75.5	42.5	10.5	28.8	33.0	52.1	1.8	s.w. & e.	..	49	12.1	4.6	0.9	0.896	5.5	529	80
Wharfedale	29.439	55.5	76.7	37.5	14.1	34.9	39.2	50.6	1.2	s.w. & s.e.	6.7	35	7.3	4.3	0.9	0.820	5.2	523	340
Newcastle	29.532	56.2	76.0	41.0	12.4	30.5	35.0	51.5	1.2	s.w. & n.e.	..	36	13.3	4.5	0.8	0.808	5.4	529	121
Number of columns	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19

latitudes of 53° and 54° was $17^{\circ}\cdot6$; at Liverpool and Whitehaven was $10^{\circ}\cdot3$; and at Durham and Newcastle was $13^{\circ}\cdot3$.

The greatest mean daily ranges of the temperature of the air took place at Hartwell, Cardington, Highfield House, Notts, and Latimer Rectory; and the least occurred at Torquay, Guernsey, Liverpool, and Truro.

The highest thermometer readings during the quarter were 91° at Wakefield; 87° at Chiswell Street, London; $86^{\circ}\cdot2$ at St. John's Wood; 86° at Leicester. The lowest thermometer readings were 29° at Beckington, 33° at Wakefield and at Leicester, and $33^{\circ}\cdot8$ at Highfield House, Notts. The extreme range of temperature of the air during the quarter in England was therefore about 56° , considering the two extremes as 86° and 30° .

Rain has fallen on the greatest number of days at Stonyhurst, Highfield House, Notts, Wakefield and Stone. The average number at these places was 51. It fell on the least number of days at Southampton, Oxford, Beckington and Greenwich, and the average number at these places was 29° . The stations at which the largest falls have taken place are Stonyhurst, Newcastle, Whitehaven and Helston. The smallest falls occurred at Beckington, Wakefield, Holkham and Norwich. The average fall in the counties of Cornwall and Devonshire was 9·8 inches; south of latitude 52° was 6·9 inches; between the latitudes of 52° and 53° was 6·9 inches; between 53° and 54° was 9·3 inches; at Liverpool and Whitehaven was 11·5 inches; and at Durham and Newcastle was 10·3 inches.

The numbers in the columns 15 to 19 show the mean values of the hygrometrical results; from which we find that—

The mean weight of vapour in a cubic foot of air at all places (excepting Cornwall and Devonshire) in the quarter ending June 30, 1849, was 4·5 grains.

The mean additional weight required to saturate a cubic foot of air in the quarter ending June 30, 1849, was 1·1 grain.

The mean degree of humidity (complete saturation = 1) in the quarter ending June 30, 1849, was 0·802.

The mean amount of vapour mixed with the air would have produced water, if all had been precipitated at one time on the surface of the earth, to the depth of 5·5 inches.

The mean weight of a cubic foot of air under the mean pressure, temperature and humidity, was 526 grains at the average height of 155 feet.

And these values for Cornwall and Devonshire were 4·8 grains; 1·2 grain; 0·808; 6·0 inches; and 528 grains, at the average height of 123 feet.

The following table exhibits the meteorological particulars of the first three quarters of the years 1847, 1848 and 1849.

Comparison of Meteorological Particulars of the Quarters ending March 31, June 30, and September 30, in the Years 1847, 1848 and 1849, in different Parallels of Latitude.

For the Counties of Cornwall and Devonshire.																				
For the quarter ending	Mean pressure of dry air reduced to the level of the sea.	Mean elastic force of vapour.	Mean temperature of the air.	Mean highest readings of the thermometer.	Mean lowest readings of the thermometer.	Mean daily range of temperature.	Mean monthly range of temperature.	Mean quarterly range of temperature.	Mean dew-point.	Wind.		Mean amount of cloud.	Rain.		Mean weight of a cubic foot of vapour.	Mean additional weight of a cubic foot of vapour.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Mean height of stations above the level of the sea.
										Mean estimated strength.	General direction.		Mean number of days on which it fell.	Mean amount collected.						
March 31. 1847. 1848. 1849.	in. 29-706 29-512 29-837	in. 0-275	41-6 42-1 45-1	56-6 59-9 57-8	25-0 24-4 29-7	° 9-2 10-4	° 23-8	31-6 35-5 28-1	° 41-1	1-8 1-6 1-3	n.e. s.w. s.w.	6-4 7-0 6-5	41 59 47	in. 9-0 12-0 7-2	grs. 2-6 3-1 3-2	grs. 0-8 0-5 0-5	0-763 0-863 0-878	in. 3-0 3-7 3-8	grs. 545 541 546	feet. 123 122 123
South of latitude 52°, exclusive of Cornwall and Devonshire.																				
1847. 1848. 1849.	29-706 29-512 29-837 0-243	36-9 39-3 41-5	62-6 66-5 60-9	8-5 15-5 20-5 10-2 13-5 32-4	54-1 50-9 40-3 37-6 1-7 1-3	n.e. s.w. s.w.	6-9 7-6 7-2	30 52 37	4-5 7-1 5-1	2-4 2-8 2-7	0-4 0-4 0-4	0-867 0-896 0-863	2-8 3-3 3-3	553 544 547 158 165
Between the latitudes of 52° and 53°.																				
1847. 1848. 1849.	29-706 29-512 29-837 0-235	37-2 39-0 40-8	61-0 63-5 58-9	20-1 17-7 16-0 11-0 11-8 34-0	40-9 45-6 42-9 36-6	1-6 1-9 1-6	e. s.w. s.w.	6-9 6-8 6-7	35 46 31	5-0 7-3 4-0 2-7 2-6 0-4 0-5 0-882 0-863 3-2 3-2 544 550 179 109

Between the latitudes of 53° and 54°.

1847.	29-706	37-3	61-7	19-0	42-7	1-3	e.	6-2	44	5-5	2-6	0-6	0-842	3-0	548
1848.	29-512	38-3	62-3	11-8	10-7	50-4	1-0	s.w.	7-7	59	12-0	2-7	0-3	0-911	3-2	542
1849.	29-837	0-242	39-9	59-8	17-3	11-1	34-1	42-4	37-5	w.	7-0	43	5-9	2-8	0-4	0-846	3-4	548

Near the sea coast on the West of England, between the latitudes of 53½° and 54½°.

1847.	29-706	38-4	59-6	24-9	34-7	1-4	s.e.	40	4-9	2-6	0-7	0-838	3-0	547
1848.	29-512	40-7	55-0	17-9	7-4	37-2	1-7	w.s.w.	6-3	56	12-0	2-7	0-3	0-878	3-2	546
1849.	29-837	0-235	41-2	54-8	21-3	6-7	26-1	33-5	36-6	s.w. & n.w.	6-8	45	6-2	2-8	0-4	0-887	3-3	550

North of latitude 54°.

1847.	29-706	36-6	61-7	19-1	42-6	1-7	variable.	28	2-8	2-4	0-5	0-844	2-7	547
1848.	29-512	38-1	57-8	6-6	9-4	51-2	1-9	s.w.	6-4	39	9-7	2-7	0-2	0-957	3-0	544
1849.	29-837	0-234	40-5	56-3	17-0	11-5	33-0	39-3	36-5	s.w.	5-7	27	3-4	2-7	0-4	0-860	3-2	545

For the Counties of Cornwall and Devonshire.

June 30.	29-603	51-3	72-8	31-2	41-4	1-6	s.w.	6-2	37	7-7	4-8	0-6	0-816	4-7	530
1847.	29-554	54-1	74-9	32-4	18-4	42-5	1-7	n.e.	4-4	40	8-7	3-8	1-2	0-765	4-7	534
1849.	29-541	0-301	52-0	75-2	32-2	14-8	32-8	43-0	43-7	n.	5-4	41	8-1	3-5	0-9	0-749	4-3	534

South of latitude 52°, exclusive of Cornwall and Devonshire.

1847.	29-603	51-7	83-1	24-4	59-4	2-3	s.w.	5-7	40	4-3	3-7	0-6	0-775	4-5	531
1848.	29-544	54-2	80-5	28-9	20-2	51-6	1-5	e.	5-5	43	7-8	3-9	1-2	0-768	4-5	531
1849.	29-541	0-298	52-1	82-4	26-8	19-9	42-4	45-1	43-4	n.n.e.	6-6	44	6-7	3-8	1-2	0-760	4-3	536

Between the latitudes of 52° and 53°.

1847.	29-603	51-1	78-9	26-9	52-1	0-9	s.w. & n.w.	6-0	45	6-3	3-8	0-8	0-825	4-6	531
1848.	29-544	53-7	81-5	32-5	20-1	50-9	1-4	s.w.	5-7	43	7-3	4-0	1-1	0-804	4-8	537
1849.	29-541	0-285	49-9	77-3	25-7	17-3	38-8	51-6	42-2	n.e.	6-2	44	7-4	3-6	0-9	0-792	4-1	537

TABLE (continued).

Between the latitudes of 53° and 54°.

For the quarter ending	Mean pressure of dry air reduced to the level of the sea.	Mean elastic force of vapour.	Mean temperature of the air.	Mean highest readings of the thermometer.	Mean lowest readings of the thermometer.	Mean daily range of temperature.	Mean monthly range of temperature.	Mean quarterly range of temperature.	Mean temperature of the dew-point.	Wind.		Mean amount of cloud.	Rain.		Mean additional weight of a cubic foot of vapour.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Mean height of stations above the level of the sea.
										Mean estimated strength.	General direction.		Mean number of days on which it fell.	in.					
une 30.	in.	in.	51.4	82.7	26.0	°	°	56.8	°	2.8	w.	5.3	50	8.7	grs.	0.736	in.	grs.	feet.
1847.	29.603	51.4	82.7	26.0	56.8	2.8	w.	5.3	50	8.7	3.8	0.7	4.6
1848.	29.544	52.3	81.2	25.8	19.7	55.4	1.1	w.	6.5	51	7.8	3.7	0.780	4.4	533	139
1849.	29.541	0.298	50.0	77.0	25.9	17.9	42.5	51.1	43.4	1.2	w.	5.8	49	5.5	3.4	0.803	4.2	536	139

Near the sea coast in the West of England, between the latitudes of 53½° and 54½°.

1847.	29.603	50.9	77.1	29.6	47.5	1.6	s.w. & n.w.	6.6	44	7.8	3.4	0.9	0.865	4.2	37
1848.	29.544	52.2	72.5	32.9	12.2	39.6	1.4	n.w. & s.w.	5.9	44	6.6	3.7	0.9	0.809	4.4	534
1849.	29.541	0.290	50.8	72.3	30.8	12.1	31.4	41.5	42.7	s.w. & n.w.	6.0	40	5.6	3.2	1.0	0.760	4.1	537

North of latitude 54°.

1847.	29.603	47.9	73.8	29.7	44.1	1.9	s.w. & n.w.	5.9	45	5.3	3.7	0.3	0.836	4.3	230
1848.	29.544	50.7	77.9	25.5	14.9	52.5	1.4	s.w.	6.0	39	5.9	3.7	1.1	0.833	4.6	530
1849.	29.541	0.292	48.4	71.0	26.0	13.5	32.5	45.0	42.9	s.e. & n.e.	39	9.9	3.4	0.7	0.834	4.1	537

XLVII. *Researches on the Theory of the principal Phænomena of Photography in the Daguerreotype Process.* By A. CLAUDET*.

ALTHOUGH the Daguerreotype process has during the last ten years been investigated by a great number of philosophers, and brought to a considerable degree of perfection by a still greater number of practitioners, it may appear surprising that the principal phænomena upon which this new art is founded, are still enveloped in a mysterious darkness.

My constant endeavour has been to explain them, and at the two last meetings of the British Association I have had the honour of communicating the results of some of my researches.

The phænomena which have not yet been satisfactorily explained, and of which I shall have to treat in the present paper, are those referring to the following points:—

1. What is the action of light on the sensitive coating?
2. How does the mercurial vapour produce the Daguerreotype image?
3. Which are the particular rays of light that impart to the chemical surface the affinity for mercury?
4. What is the cause of the difference in achromatic lenses between the visual and photogenic foci? why do they constantly vary?
5. What are the means of measuring the photogenic rays, and of finding the true focus at which they produce the image?

At the last meeting of the British Association, which took place at Swansea, I announced that the decomposition of the chemical surface of the Daguerreotype plate by the action of certain rays of light produced on that surface a white precipitate, insoluble in the hyposulphite of soda, which, when examined by the microscope, had the appearance of crystals reflecting light, and which, when seen by the naked eye, were the cause of a positive Daguerreotype image.

This fact had not been observed before. The opinion of Daguerre himself and other writers was, that the action of light on the iodide of silver had only the effect of darkening the surface, and consequently of producing a negative image. But it escaped them, that, under the darkened iodide of silver, another action could take place after a continued exposure to light, and that the hyposulphite of soda washing could disclose a positive image. I have proved this unexpected fact in obtaining, by the action of light only, and without mercury,

* Communicated by the Author, having been read before the British Association at Birmingham, Sept. 14, 1849.

images having the same appearance as those developed under the action of mercurial vapour. This direct and immediate effect of light is certainly remarkable; but the Daguerreotype process is not founded on that principle on account of the slowness of its action; and it is fortunate that, long before light can produce the white precipitate I have alluded to, it operates another effect, which is the wonderful property of attracting the vapour of mercury. This vapour is condensed in the form of a white powder, having also, when examined by the microscope, the appearance of reflecting crystals. The Daguerreotype image is due to this property, which is the most beautiful feature of Daguerre's discovery.

M. Moser has given an ingenious theory of the action of mercury. Knowing that the yellow ray had the property of continuing the effect commenced by light on the iodide of silver, he has supposed that mercury, when in a state of vapour, evolves a latent yellow light, and to the action of that yellow light of mercurial vapour he ascribes the continuation of the decomposition of the iodide of silver. But as the analysis of the surface discloses the presence of mercury, that metal must have been amalgamated with the silver set free after the action of light. We must therefore look for another explanation of the phænomenon.

It is more probable that light exercises a twofold action on the iodide of silver, whether it is combined or not with chlorine or bromine. By one, the iodide is decomposed, and the silver set free is precipitated on the surface in the form of a white powder or small crystals; by the other, which begins long before the former, the parts affected by light have been endowed with an affinity for mercurial vapour.

By means of my photographometer, to the principle of which I shall presently refer, I have been able to ascertain that the pure light of the sun performs in about two or three seconds the decomposition of the bromo-iodide of silver, which is manifested by the white precipitate; while the same intensity of light determines the affinity for mercurial vapour in the wonderfully short space of about $\frac{1}{1000}$ th part of a second. So that the affinity for mercury is imparted by an intensity of light 3000 times less than that which produces the decomposition manifested by the white precipitate.

For this reason it is difficult to suppose that the two actions are the same. We must admit that they are different. Long before it can effect the decomposition of the surface, light imparts to the sensitive coating the affinity for mercurial vapour; and this appears to be the principle of the formation of the image in the Daguerreotype process.

In a paper I communicated to the Royal Society on the 17th of June 1847 (see Transactions), and an abstract of which I read before the Association at Oxford, I stated that the red, orange and yellow rays were destroying the action of white light, and that the surface was recovering its former sensitiveness or unaffected state after having been submitted to the action of these rays. I inferred from that curious fact that light could not have decomposed the surface; for if it had, it would be difficult to understand how the red, orange, or yellow rays could combine again, one with another, elements so volatile as bromine and iodine, after they had been once separated from the silver.

But I had not yet been able to ascertain that, when light has decomposed the bromo-iodide of silver, the red, orange or yellow rays cannot restore the surface to its former state. The action of light, which can be destroyed by the red, orange or yellow rays, does not determine the decomposition, which would require an intensity 3000 times greater. It is the kind of action produced by an intensity 3000 times less, giving the affinity for mercury, which is completely destroyed by the red, orange or yellow rays. It seems, therefore, that I was right in saying that there was no decomposition of the compound during the short action which is sufficient to give the affinity for mercury, and in ascribing the formation of the image only to that affinity. White light, or the chemical rays which accompany it, communicate to the surface the affinity for mercury, and the red, orange, or yellow rays withdraw it. I must notice here a singular anomaly; viz. that when the sensitive surface is prepared only with iodine without bromine, the red, orange or yellow rays, instead of destroying the action of white light, continue the effect of decomposition as well as that of affinity for mercury. Still there is a double compound of iodine which is far more sensitive than the simple compound, and on which the red, orange, or yellow rays exercise their destructive action as in the case of the bromo-iodide.

The phænomenon of the continuing action of the red, orange or yellow rays, on the simple compound of iodide of silver, was discovered by M. Ed. Becquerel; and soon after M. Gaudin found, that not only those rays continue the action by which mercury is deposited, but that they develop without mercury an image having the same appearance as that produced by mercurial vapour.

M. Gaudin, not having observed the fact of the white precipitate, which is the result of the decomposition by the action of light, could not explain the cause of the image brought out under the influence of the yellow ray.

I have observed that the iodide of silver without bromine is about 100 times more sensitive than the bromo-iodide to the action of light, which produces the decomposition of the compound forming the white precipitate of silver, while it is 100 times less sensitive for the effect which gives the affinity for mercury. This seems another reason for supposing that the two actions are different. It may be that, in the case of the iodide of silver alone, the decomposition being more rapid, and the affinity for mercury slower than when bromine is added to the compound, the red, orange, and yellow rays having to act upon an incipient decomposition, have the power, by their own photogenic influence, of continuing the decomposition when it has begun. This may explain the development of the image under red, orange, or yellow glasses, according to M. Gaudin's discovery. But in the case of the bromo-iodide of silver, the red, orange, or yellow rays have to exert their action on the affinity for mercury, begun a long time before the decomposition of the compound; and they have the property of destroying that affinity.

So that it would appear that all the rays of light have the property of decomposing the iodide of silver in a longer or shorter time, as they have that of producing the affinity for mercury on the bromo-iodide of silver; with the difference, that on the former compound the separate actions of the several rays continue each other, and that on the second compound these separate actions destroy each other. We can understand that, in the first case, all the rays are capable of operating the same decomposition; and that in the second, the affinity for mercury when imparted by one ray is destroyed by another. This would explain the various phenomena of the formation of the two different deposits I have described, and also explain the anomaly of the continuation of the action of light by the red, orange, or yellow rays, according to M. Ed. Becquerel's discoveries on the iodide of silver; and of the destruction of that action by the same rays, according to my own observations on the bromo-iodide of silver.

The red, orange and yellow rays, when acting on an unaffected surface, are considerably less capable than the most refrangible rays of imparting the affinity for mercurial vapour on both the iodide and bromo-iodide of silver; and they destroy that affinity when it has been produced on the bromo-iodide of silver by the photogenic rays. It follows from this fact, that when the red, orange, or yellow rays are more abundant in the light than the most refrangible rays, the photogenic effect is retarded in proportion to the excess of these antagonistic rays. This happens when there exists in the

atmosphere some vapours which absorb the most refrangible rays. In these circumstances the light appears rather yellow; but it is very difficult to judge by the eye of the exact colour of the light, and of the proportion of photogenic rays existing in the atmosphere at any given moment.

The vapours of the atmosphere which render the light yellow, act as any other medium intercepting the blue rays, and those which have the same degree of refrangibility. I prove, by a very simple experiment, the comparative photogenic action of rays which have passed through such media, and of those which have met with no similar obstacle; also that media which intercept the photogenic rays can let pass freely the illuminating rays.

If I cover an engraving one-half with light yellow glass, and place it before my camera obscura in order to represent the whole on a Daguerreotype plate, I find that during the time which has been necessary to obtain the image of the half not covered, not the slightest effect has been produced on the half covered with the yellow glass.

Now if I cover one half with deep blue glass and the other with the same light yellow glass, the engraving will be seen very distinctly through the yellow glass, and not at all through the blue. In representing the whole, as before, on the Daguerreotype plate, the half which was clearly seen by the eye has produced no effect; and the other, which could not be seen, is as fully represented, and in nearly as short a time, as when no blue glass had been interposed.

Thus we might construct a room lighted only through an inclosure of light yellow glass, in which light would be very dazzling to the eye, and in this room no photographic operation could be performed; or a room inclosed by deep blue glass, which would appear very dark, and in which the photographic operation would be nearly as rapid as it would be in open air.

Thus we may conceive certain states of the atmosphere under which there will be an abundance of illuminating rays, and very few photogenic rays; and some others, under which the reverse will take place.

Considering how difficult it is to judge by the eye alone of the photogenic state of light, we can understand why the photographer is constantly deceived in the effect he tries to produce, having no means to ascertain beforehand, with any degree of certainty, the intensity of light. For these reasons I turned my attention to contrive an apparatus by which I could test at the same time the sensitiveness of the Daguerreotype plate and the intensity of light.

I succeeded in constructing an instrument which I have called a photographometer, the description of which appeared in the *Philosophical Magazine* for the month of November 1848.

As I have since improved it considerably, and made with it a great number of experiments, I shall briefly refer to this instrument, and describe the useful alterations I have made.

In the instrument described in the *Philosophical Magazine* for November 1848, the light struck the Daguerreotype surface during the passage on an inclined plane of a metallic plate having seven apertures in a horizontal line, following the geometrical progression 1, 2, 4, 8, 16, 32, 64; so that the Daguerreotype plate being covered with another metallic plate having four series of seven holes, the effect of light through every one of the seven holes was represented in proportion to the opening of the moveable plate. Every one of the four series of holes indicated the same number of white spots, and the number of spots was the measure of the light at the moment. I had four series of holes in order to try several preparations on the same plate, or to test the light on the same plate at four different times.

The improvement I have made consists in my being able to shut every one of the holes by means of sliding blades; so that I can continue, by repeated falls, the geometrical progression from 1 to 512 on one plate; and when a second plate is added to the double apparatus, from 1 to 8192. This enables me to compare and follow the different effects of light in a considerable range of intensities. This is done in the following manner:—After having given one fall with all the slides open, I shut one and give another fall, then shut the second slide and give two falls, and so on, always doubling the number of falls for every new slide shut.

It is by this means that I have been able to discover at what degree of intensity of light the effect called solarization is produced;—on well-prepared plates of bromo-iodide it does not begin under an intensity 512 times greater than that which determines the first effect of mercury;—and also at what degree the decomposition producing the white precipitate without mercury manifests itself, both on iodide and on bromo-iodide of silver. On the first, it is 100 times quicker than on the bromo-iodide; and on the last, it is produced by an intensity 3000 times greater than that which develops the first affinity for mercury.

The slides enable me to try the effect of different insulated rays on plates affected by white light. This is done by shutting one-half of each hole in pushing the sliding blades just

enough for that purpose. In that state I submit the surface acted on by a great number of intensities of light to the subsequent radiation through red, orange, or yellow glasses, or any other coloured transparent media, in order to examine the action of these radiations on one-half of the effects produced by each intensity of light. By these means I have found, that before light has decomposed the surface and produced the white precipitate, the red, orange, and yellow rays destroy the affinity for mercury, and continue it when the decomposition has begun.

In the course of my experiments I noticed a curious fact, which proved very puzzling to me, until I succeeded in assigning a cause to it. I shall mention it here, because it may lead to some further discoveries. I observed that sometimes the spaces under the round holes, which had not been affected by light during the operation of the photogrophometer in a sufficient degree to determine the deposit of mercury, were, as was to be expected, quite black; while the spaces surrounding them were in an unaccountable manner slightly affected by mercury. At first I could not explain the phænomenon, except by supposing that the whole plate had been previously by accident slightly affected by light, and that the exposure through the holes to another sort of light had destroyed the former effect. I was naturally led to that explanation, having before observed that one kind of light destroys the effect of another; as, for example, that the effect of the light from the north is destroyed by the light from the south, when certain vapours existing in the latter portion of the atmosphere impart a yellow tint to the light of the sun. But after repeated experiments, taking great care to protect the plate from the least exposure to light, and recollecting some experiments of M. Moser, I found that the affinity for mercury had been imparted to the surface of the Daguerreotype plate by the contact of the metallic plate having the round holes, while the space under the hole had received no similar action. But it must be observed that this phænomenon does not take place every time; some days it is frequent, and in some others it does not manifest itself at all. Considering that the plate furnished with round holes is of copper, and that the Daguerreotype plate is of silver plated on copper, it is probable that the deposit of mercury is due to an electric or galvanic action determined by the contact of the two metals; and perhaps the circumstance that the action does not take place every time, will lead to the supposition that it is developed by some peculiar electric state of the ambient atmosphere; and by a degree of dampness in the air, which would increase the

electric current. May we not hope that the conditions being known in which the action is produced, and by availing ourselves of that property, it will be possible to increase on the Daguerreotype plate the action of light? for it is not improbable that the affinity for mercury imparted to the plate is also due to some electrical influence of light. How could we otherwise explain that affinity for mercury given by some rays and withdrawn by some others, long before light has acted as a chemical agent?

Photography is certainly one of the most important discoveries of our age. In relation to physics and chemistry, it has already been the means of elucidating many points which had not been investigated, or which were imperfectly known before. We may certainly expect that its study will prove of considerable use to the progress of these sciences. But it is in reference to optics that it opens a large field for research and discovery. Had Newton been acquainted with the properties with which light is endowed in the phænomena of photography, there is no doubt he would have left a more complete theory of light, and of the various rays which compose it.

Since the discovery of photography, opticians have turned their attention to the constructing of new combinations of lenses, in order to increase the illuminating power without augmenting the aberration of sphericity. It is due to justice to state here, that the optician who first produced the best lenses for photography is M. Voigtlander of Vienna, and they still are the most perfect that a photographer can use, particularly for portraits. In this country an optician of great merit, Mr. A. Ross, has constructed lenses on similar principles; and at all events has succeeded in producing some which work as quick, and give an image as perfect in every respect. In Paris M. Lerebours is renowned for lenses with larger focus, which are better adapted for taking views than any I have tried.

From the beginning of photography it was well known that the effective rays being the most refrangible, had a shorter focus than those producing white light; and for this reason Daguerre himself recommended the use of achromatic lenses, in which all the rays were supposed to coincide nearly at the same focus. All camerae obscuræ were furnished with achromatic lenses, and constructed so that the plate could be placed exactly at the same distance as the ground glass on which the image had appeared the best defined. But with these camerae obscuræ it was very difficult to obtain a photographic image so perfect as that seen on the ground glass; and it was only now and then, and as if by accident, that good pictures could be produced.

I soon observed that anomaly, and imagined that it was due to some errors in the respective position of the two frames; one holding the ground glass, and the other containing the plate, which, by warping or some other causes, might have been shifted to different distances from the object-glass.

Not being able to assign another reason for the error, I constructed a camera obscura in which the ground glass and the plate were exactly placed in the same frame. In doing so I hoped to avoid the least error or deviation. But to my surprise, the more I was correct in my adjustment, the less I could obtain a well-defined Daguerreotype picture. This proved to me that I had to seek for another cause of the difficulty; and before going any further, I decided to try if the usual focus did or did not really coincide with the photogenic focus. For the experiment, I placed at a distance from the camera obscura several screens on different planes: these screens being covered with black lines, I could see them very distinctly on the ground glass. I tried the focus on one of the screens. To my surprise and delight, I found that invariably the one which had come out well-defined on the ground glass was confused on the Daguerreotype plate, and *vice versa*. This was sufficient to prove to me the cause of the difficulty I had been labouring under, viz. that the visual focus had not coincided with the photogenic focus. But the most surprising feature of that discovery was, that the photogenic focus was longer than the visual focus. On first consideration it should have been shorter, as the rays operating in photography are the most refrangible. Although I could not at first understand the cause of this anomaly, it was sufficient for me to know that, in order to have a well-defined Daguerreotype picture, I had only to set the focus on the ground glass for an object nearer the camera at the distance indicated by the experiment with the various screens. Continuing my experiment, I found some lenses in which the photogenic focus was shorter, and some others in which the two coincided.

I communicated a paper on the subject to the Royal Society and to the Académie des Sciences in May 1844, and from that time photographers have been able to find the true photogenic focus of their camera; and opticians, who at first denied the fact, have at last studied and considered the question, trying to construct lenses in which the two foci should agree.

M. Lerebours of Paris was the first who, on my suggestion, examined the subject; and he communicated a paper to the Académie des Sciences, in which he explained the cause of the difference. He stated that, by altering the proportion

between the angles inscribed in the curves either of the crown- or flint-glass, he could render at will the photogenic focus longer or shorter than the visual focus, and by the same means could bring them to the same point. There is no question that M. Lerebours was right as far as the result referred to the chromatic correction ; but if, according to the density of the two glasses, certain curvatures are required to correct the spherical aberrations, these curvatures cannot be altered with impunity only for the purpose of changing the directions of the most refrangible rays. For this reason I have always preferred lenses in which the spherical aberration is the most perfectly corrected, without caring whether the photogenic rays coincided or not with the visual rays, having the means of ascertaining how I could obtain on my Daguerreotype plate the best-defined image. In fact, from my own observation that the red, orange, and yellow rays are antagonistic to the photogenic rays, and that the last rays have a greater power when the former are proportionately less abundant, I am of opinion that when the photogenic rays are only condensed on the plate, and the others are dispersed on the space more or less distant from the photogenic point, the action is more rapid. Rapidity being the principal object in photography, I prefer lenses in which the two foci are separated, although the operation is a little more difficult, and requires considerable care.

The question of the photogenic focus is involved in another kind of mystery, which requires some attention. I have found that with the same lenses there exists a constant variation in the distance between the two foci. They are never in the same relation to each other : they are sometimes more or less separate ; in some lights they are very distant, and in some others they are very near and even coincide. For this reason I constantly try their position before I operate. I have not been able to discover the cause of that singular phænomenon, but I can state positively that it exists. At first I thought that variations in the density of the atmosphere might produce the alteration in the distance between the two foci ; or that when the yellow rays were more or less abundant, the visual rays were refracted on different points on the axis of the foci, according to the mean refrangibility of the rays composing white light at the moment. But a new experiment has proved to me that these could not be the real causes of the variation. I generally employ two object-glasses ; one of shorter focus for small pictures, and the other of longer focus for larger images. In both the photogenic focus is longer than the visual focus ; but when they are much separated in one they are less so in the other : sometimes when they coincide in one, they are

very far apart in the other, and sometimes they both coincide. This I have tried every day during the last twelve months, and I have always found the same variations. The density of the atmosphere, or the colour of light, seems to have nothing to do with the phænomenon, otherwise the same cause would produce the same effect in both lenses. I must observe that my daily experiments on my two object-glasses are made at the same moment and at the same distance for each, otherwise any alteration in the focal distance would disperse, more or less, the photogenic rays, which is the case, as I have ascertained. The lengthening or shortening the focus, according to the distance of the object to be represented, has for effect to modify the achromatism of the lenses. An optician, according to M. Lerebours's calculation, can at will, in the combination of the two glasses composing an achromatic lens, adapt such curvatures or angles in both that the visual focus shall coincide with the photogenic focus; but he can obtain this result only for one length of focus. The moment the distance is altered, the two foci separate, because the visual and photogenic rays must be refracted at different angles in coming out of the lens, in order to meet at the focus given for one distance of the object. If the distance is altered, the focus becomes longer or shorter; and as the angle at which different rays are refracted remains nearly the same, they cannot meet at the new focus, and they form two images. If the visual and photogenic rays were refracted parallel to each other, in coming out of the lens they would always coincide for every focus; but this is not the case.

It seems, therefore, impossible that lenses can be constructed in which the two foci will agree for all the various distances, until we have discovered two kinds of glasses, in which the densities will be in the same ratio as their dispersive power. There is no question so important in photography as that which refers to finding the true photogenic focus of every lens for various distances. I have described the plan I have adopted for that purpose; by means of that very simple instrument, every photographer can always obtain well-defined pictures with any object-glasses. But there is another method of ascertaining the difference between the two foci, which has been lately contrived by Mr. G. Knight of Foster Lane, London. That gentleman has been kind enough to communicate to me the very ingenious and simple apparatus, by which he can at once find the exact difference existing between the visual and photogenic focus, and place the Daguerreotype plate at the point where the photogenic focus exists. I am very glad he has entrusted me with the charge of bringing his invention before the British Association. For the scientific

investigation of the question Mr. Knight's apparatus will be most valuable to the optician, as it will afford him the means of studying the phænomenon with mathematical accuracy.

Mr. Knight's apparatus consists in a frame having two grooves; one vertical, in which he places the ground glass, and the other forming an angle with the first destined to receive the plate; the planes of the grooves intersect each other in the middle. After having set the focus upon the ground glass, this last is removed, and the plate is placed in the inclined groove. Now if a newspaper or any large printed sheet is put before the camera, the image will be represented on the inclined plate; and it is obvious in its inclination the various points of the plate will meet a different focus; the centre of the plate will coincide with the visual focus by its inclination. It will in one direction meet the photogenic focus at a point more or less distant from the centre, if the photogenic focus is shorter than the visual focus, and in the other direction if it is longer. The frame is furnished with a scale of division, having the zero in the centre. When the image is represented on the Daguerreotype, by applying against it another moveable scale of division similar to the other, the operator can find what is the division above or under zero at which the image seems best defined; and after having removed from the camera the experiment frame, and set the focus as usual on the ground glass, he has only to move the tube of the object-glass by means of the rack and pinion, and to push it in or out, a space corresponding with the division of the scale indicating the deviation of the true photogenic focus: the tube of the object-glass is for that purpose marked with the same scale of division.

In order to enable the members of the Association to judge of the merit of Mr. Knight's invention, I have had his apparatus applied to a small camera with which I made my experiment. By exhibiting at the same time Mr. Knight's method and my own, a comparison of the two may be made, and they will be both better understood.

Before concluding, I shall call the attention of all persons conversant with optics to the singular fact I have observed respecting the constant variation of the two foci. I have not been able yet to find its cause, and I leave its investigation to more competent persons. I hope at the next meeting of the Association we shall know more on the subject.

XLVIII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 308.]

May 11, **O**N the Determination of the most probable Orbit of a 1849. Binary Star from the assemblage of a great number of observed Angles of Position. By Sir J. F. W. Herschel, Bart. With some Remarks by the President on a Solution of the same Problem by M. Yvon Villarceau.

In this paper Sir John Herschel refers generally, for the principle of his method, to a paper published by him in the fifth volume of the Memoirs of this Society, the paper (we may remark) in which was given an exposition of the principles by which the orbit of a double star was for the first time actually determined. He now states his conviction that the method there expounded is, on the whole, the best that can be employed; and the object of the present paper is, retaining the original principle (namely, of using only the measured angles of position, and rejecting entirely the measures of distance), and retaining the first step of the original method (namely, of smoothing down the irregularities of the angles as measured, by laying them down graphically, the angles for abscissæ and the corresponding times for ordinates, and then drawing a curve by hand through the points so found, and using that curve as the representation of the real relation between the angles and the times, and measuring from it the times corresponding to angles which differ by 5° , or by 10° , or any other convenient difference); retaining the original method thus far, to complete the investigation by a process entirely algebraical and arithmetical.

Supposing the times corresponding to equal intervals of angle to be taken from the curve above mentioned, the next thing required is $\frac{dt}{d\theta}$ for every 5° or every 10° , &c. This is to be found by the following formula, which requires for application only the finite differences of t for the equidistant values of θ ,

$$\frac{dt}{d\theta} = \frac{1}{\Delta\theta} \left\{ \frac{\Delta t}{1} - \frac{\Delta^2 t}{2} + \frac{\Delta^3 t}{3} - \&c. \right\}$$

The next step is, to infer from this the true apparent distance of the stars, as it ought to be measured by a perfect micrometer or measuring instrument. Now every determination of an orbit of double stars proceeds on the assumption of an attraction between the two components, and this requires the supposition of description of areas proportional to the time, both in the orbit really described and in the projection of the orbit which we see. Hence we must have $\rho^2 \cdot \frac{d\theta}{dt} = \text{constant} = 100$ (the unit of the radius vector

being for the present arbitrary), and therefore $\rho = \sqrt{100 \cdot \frac{dt}{d\theta}}$ or $= \sqrt{-100 \frac{dt}{d\theta}}$, according as $\frac{d\theta}{dt}$ is positive (that is, θ increasing in the

direction *nfs*p) or negative. Sir John adopts for the unit of angles one degree, and for the unit of time one year.

A series of radii vectores being thus found, corresponding to certain values of θ , the next step is to form from these in numbers the corresponding values of the rectangular co-ordinates $x = \rho \cdot \cos \theta$ $y = \rho \cdot \sin \theta$. And, assuming that the force of attraction between the two stars follows the law of the inverse square of the distance, and therefore that the curve really described is a curve of the second order, and consequently that the apparent curve is a curve of the second order, we must make these numerical values of x, y , (as $x_1 y_1, x_2 y_2, x_3 y_3$, &c.) satisfy the equation $0 = 1 + \alpha x + \beta y + \gamma x^2 + \delta xy + \varepsilon y^2$, an equation containing 5 unknown constants. As the number of equations will generally exceed 5, it will be proper to combine them by the method of least squares; and the only question is, what is the function of x and y which shall be supposed *à priori* liable to equal error in all? Sir John Herschel tacitly assumes that the function $B = 1 + \alpha x + \beta y + \gamma x^2 + \delta xy + \varepsilon y^2$ is the quantity which with equal weight throughout is to be made as small as possible, or that $\Sigma(B^2)$ is to be minimum. The equations given by this consideration are easily formed, and then $\alpha, \beta, \gamma, \delta, \varepsilon$ can be determined.

From these numbers the numerical values of the more convenient elements of the apparent ellipse may be found, and from them the elements of the real ellipse may be found. The formulæ for all these transformations are given at length by Sir John Herschel, and they are less complicated than might at first have been feared.

Thus far the elements necessary to produce geometrical coincidence of the concluded orbit with the observed orbit are alone determined. The next operation is to determine those elements which relate to the motion in the concluded orbit. For this purpose, angles being taken from the curve based on the graphical projection, and these angles (which relate to the apparent orbit) being converted into angles in the true orbit by the formulæ lately found, and thus exhibiting true anomalies on the true ellipse, the excentric anomalies are found at once by the formula $u - e \sin u$, and the mean anomalies are found. Then every one of these angles gives an equation of the form

$$w_1 = k.t_1 - l,$$

from the assemblage of which the constants k and l can be found by the method of least squares; and then we have all that is required to form the mean anomaly for any other time t , and consequently (as the elements of the ellipse are known) to form the excentric and true anomalies.

The conversion of a place thus computed in the real orbit into one in the apparent orbit, and the comparison of the distance computed on the arbitrary scale with the distance measured with the micrometer, and the inference as to the true value of the units of the arbitrary scale, are steps which require no particular explanation.

Sir John Herschel holds out the hope of following up this expo-

sition with the details of the application of his method to the star γ Virginis.

As an Appendix to Sir John Herschel's paper, it is proper to add that papers have been received by Sir John Herschel from M. Yvon Villarceau (namely, a note on the double star ζ Herculis, dated 1849, February 1, a note on the double star γ Coronæ, dated 1849, March 30, and a letter dated 1849, April 1, containing an exposition of M. Yvon Villarceau's methods), which have been communicated more or less completely to the Académie des Sciences of France, and which therefore cannot be received in the ordinary way as a communication to this Society. It is, however, the wish, both of Sir John Herschel and of M. Yvon Villarceau, and it appears in every way desirable that their results should be made known to this Society, both as containing instructive expositions of a very elegant general method and very curious applications of it, and also as bearing upon any questions which may arise as to the similarity or priority of the methods of Sir John Herschel and M. Yvon Villarceau.

Assuming the law of gravitation, and consequently the law of elliptic movement, as applying generally to the relative motion of two stars in a binary system, M. Yvon Villarceau remarks that the projection of this curve upon the spherical sky (or rather upon a plane perpendicular to the visual ray) will be a curve of the second order, whose equation will be,

$$F = ay^2 + bxy + cx^2 + dy + ex + f = 0,$$

the origin of co-ordinates being one star regarded as a fixed centre of attraction of the other. The object of the next process must be, to adopt this general equation to the particular observations from which the orbit is to be deduced: and here it is to be observed that M. Villarceau does not confine himself either to the measured angles of position or to the measured distances, but uses both, for the formation of the numerical values of x and y corresponding to every observation. Having these numerical values of rectangular co-ordinates, and paying no respect (for the present) to the intervals of time between the observations, the following is the method used to accommodate *geometrically* the curve of the second order to the observed co-ordinates:—

The principle assumed is, that the constants a , b , c , &c. shall be so determined that if the resulting curve be drawn, and if from every observed place a normal (usually a very short line) be drawn to the curve, then the sum of the squares of these normals, each multiplied by its proper weight, shall be a minimum. This principle, it is almost unnecessary to remark, is imperfect, inasmuch as it does not in any way take cognisance of the laws of movement, as connected with *time*; but it will frequently be doubtful, in a problem of such difficulty, whether it is not best to neglect a condition, even of the most essential kind, for the sake of making the solution more simple.

Putting $b^2 D^*$ for $\left(\frac{dF}{dx}\right)^2 + \left(\frac{dF}{dy}\right)^2$, and p for the weight of

each determination, M. Villarceau arrives thus at the following equations :—

$$\Sigma . \frac{pF \frac{dF}{da}}{D^2} = 0, \text{ or } \Sigma . \frac{py^2F}{D^2} = 0$$

$$\Sigma . \frac{pF \frac{dF}{db}}{D^2} = 0, \text{ or } \Sigma . \frac{pxyF}{D^2} = 0$$

&c.,

and he shows how, supposing an ellipse roughly drawn by hand, the value of D may be found graphically; and it will then be possible to solve the equations.

The projected ellipse being thus determined, the real ellipse will be found from the consideration that the origin of co-ordinates is the projection of the focus of the real ellipse, while the centre of the observed ellipse is the projection of the centre of the real ellipse. The formation of the corresponding equations is a not difficult problem of analytical geometry. This transformation, however, is not required till all the other operations are completed.

The points determined by observation are not generally found exactly upon the projected ellipse. In order to have points upon the ellipse which shall be the subjects of further investigation, M. Villarceau transfers the observed points to the ellipse by drawing normals to the ellipse, and taking, instead of the point actually determined by observation, the foot of its normal. If x' and y' be the co-ordinates determined from observation, x and y those of the foot of the normal, then

$$x = x' - \frac{\frac{dF}{dx}}{\left(\frac{dF}{dx}\right)^2 + \left(\frac{dF}{dy}\right)^2} \cdot F(x', y')$$

$$y = y' - \frac{\frac{dF}{dy}}{\left(\frac{dF}{dx}\right)^2 + \left(\frac{dF}{dy}\right)^2} \cdot F(x', y')$$

with sufficient exactness.

The next point is, to introduce the consideration of time; and this is to be done by making the areas described by the radius vector in the projected ellipse proportional to the time. The areas can be expressed in terms of the corrected co-ordinates and the constants without much difficulty, the whole of these admitting of further correction if necessary. M. Villarceau remarks that if there are four observed places, the solution of the four equations $F(x, y) = 0$ will give four of the quantities a, b, c, d, e , in terms of the fifth; that these four observations will give three areas between which there are two equations of proportion; and that thus, besides

the determination of the fifth coefficient, we shall have an equation of condition which must be satisfied, or whose failure will prove that our operations or assumptions are in some part erroneous. When there are more than four equations, all can be used in methods analogous to those which are well understood in other investigations, for correcting the result.

We must, however, express our opinion that this part of the operation appears the most obscure, as well as the most delicate and difficult, of the whole.

M. Villarceau remarks that the final determination of elements will in all cases require observations separated by a considerable interval from the rest.

M. Villarceau has lately communicated to the Académie another method.

The following are the principal results in the two cases which M. Yvon Villarceau has specially examined:—

In the instance of ζ Herculis, the stars are so unequal that there can be no possibility of confusion between the two. It was seen double in 1782, but there is reason to think that it was seen as only one star between 1795 and 1802, and also between 1828 and 1832. M. Struve, expressing himself very doubtful, seemed to suppose that the periodic time might be about 14 years. (See the *Mensuræ Micrometricæ*.) A valuable series of observations, however, having been made at Pulkowa, extending to 1847, the whole of which have been communicated to M. Yvon Villarceau, he has deduced from them an orbit in which the excentricity = $\sin 27^\circ$ nearly, and the periodic time is $36\frac{1}{2}$ years. The measure of 1782 and those from 1826 to 1847 appear to be represented with all desirable exactness. [In comparing the computed and observed angles of position, we are glad to see that M. Villarceau has converted their effects into expressions measured by seconds of arc.] The remarks, too, made by M. Struve about the time of the union of the two stars observed by him correspond exactly to the positions given by M. Villarceau's elements. Those of Sir W. Herschel do not correspond. M. Villarceau suggests that, at a time when the small star really was hidden, Sir W. Herschel may have been misled by a false image of the large star; and that, when the image of the star was deformed, he may have estimated the deformation in the wrong direction. He desires, however, specially to submit these conjectures to the judgement of Sir John Herschel; and we trust that Sir John Herschel will not decline to undertake the honourable task to which he is invited.

M. Villarceau concludes with pointing out that this star presents a remarkable illustration of the amount of uncertainty which may rest upon the determination of double-star elements, when based upon a limited series of observations. If we had only to satisfy the observations extending from 1828 to 1847 (or through more than one-half of a revolution), we might have represented them by systems of elements in which the excentricity varies from 0.44 to 1.63, that is, the orbit might have been an ellipse, a parabola, or a hyperbola.

In the instance of γ Coronæ there is a difficulty of a totally different kind. The two stars are so very nearly equal in magnitude and similar in colour, that, when observations are interrupted for a long time, it is impossible to say whether that which is adopted as the zero-star before and after the interruption is the same; and it is therefore necessary in some cases to make double computations, on the two suppositions that the first star, or the second star, is that to which the measures are referred in other observations.

From the observations to which they had access, M. Struve, Sir John Herschel, and M. Mädler, concluded that the periodic time of this star was 43 or 44 years. M. Villarceau, however, has had access to the observations made at Pulkowa from 1826 up to 1847, and has treated them in the following manner:—

Of fifteen observations, four were rejected, on account of manifest errors in the distance only. From the remaining eleven, relations were obtained between the elements, which leave them dependent upon an indeterminate quantity which is arbitrary between very wide limits. The observations of Sir John Herschel in 1823, and of M. Struve from 1826 to 1847, may be represented with sufficient accuracy by ellipses in which the periodic time ranges from 38 to 190 years. To fix this indeterminate quantity, we may take Sir W. Herschel's observation of 1781 or that of 1802 (with a slight alteration sanctioned by Sir John Herschel). If we fix the indeterminate quantity by the observation of 1802, M. Villarceau finds that the observation of 1781 is also satisfied, provided that the position of the stars be reversed; that is, provided that it be assumed that the other star has been used as the zero, which is perfectly admissible. Thus is obtained an orbit with a periodic time of 66 years.

But if we reverse the position of the stars in 1802, which is admissible, it is found that the observation of 1781 is satisfied without reversion. The periodic time thus obtained is 43 years.

It is remarkable that in these totally different solutions the excentricity is sensibly the same, namely, 0.47.

In both cases the remaining errors are so small, in comparison with the probable errors, as to leave the two solutions equally entitled to our reception. For the final judgement between them, M. Villarceau refers to some remarks of Sir W. Herschel, unaccompanied by measures. Although there is some doubt in the interpretation of these, M. Villarceau thinks that upon the whole the solution which gives a period of 66 years is the more probable. He remarks, however, that in four years at the furthest the doubt will be settled. In 1853.677 the angle of position given by the 66-year solution will be $303^{\circ} 44'$, while that given by the 43-year solution will be $356^{\circ} 30'$, leaving a difference upon which there can be no doubt. The distances will be respectively $0''.51$ and $0''.77$, but between these it might be difficult to pronounce.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 231.]

May 21, 1849.—On Hegel's Criticism of Newton's *Principia*.
By Dr. Whewell.

Parts of Hegel's *Encyclopædia* are here examined with the purpose of testing the value of his philosophy, not of defending Newton. Hegel says that the glory due to Kepler has been unjustly transferred to Newton: confounding thus the discovery of the laws with the discovery of the force from which the laws proceed, in which latter discovery Kepler had no share. Hegel pretends to derive the Newtonian "formula" from the Keplerian law, thus;—by Kepler's law, A being the distance, and T the periodic time, $\frac{A^3}{T^2}$ is constant: but

Newton (Hegel says) calls $\frac{A}{T^2}$ universal gravitation, whence universal gravitation is inversely as A^2 :—a most absurd misrepresentation of the course of Newton's reasoning. In the same manner Hegel criticises, and utterly misrepresents Newton's explanation, for the elliptical orbit, of the body's approaching to and receding from the centre; and of the reason why the body moves in an ellipse. Hegel also offers his own explanation of Kepler's laws from his own *à priori* assumptions. He says that the motion of the heavenly bodies is not a being pulled this way or that, as is imagined by the Newtonians; they go along, as the ancients said, like blessed gods.

XLIX. Intelligence and Miscellaneous Articles.

RAIN, THE CAUSE OF LIGHTNING.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

Leeds, October 17, 1849.

AS the whole science of meteorology depends upon the number of phænomena observed, I am led to trouble you with the following short notice of a phænomenon which particularly engaged my attention during its occurrence; if you deem the communication worthy to be inserted in your Journal, you will oblige

Your humble Servant,

T. H. DIXON.

I observed a paper upon this subject in the *Philosophical Magazine* for September last, and it reminded me that I had observed a similar phænomenon, of which I made some notes at the time of its occurrence.

On the evening of June 4, 1849, a very severe thunder-storm visited Leeds and the neighbourhood; my attention was particularly directed to this storm from a peculiar circumstance connected with

it; the storm begun about 9 P.M., and continued until 12, but the rain did not begin till 11. The whole of the shower which followed was characterized by the wave-like intensity of the falling rain; first the shower began very violently, and gradually got less intense until succeeded by another sudden increase, and during the whole storm this increase and decrease could be distinctly marked. I also noticed a similar circumstance at Redcar in July, and can most certainly bear witness that in many instances the increase of rain preceded the flash of lightning, and this occurred many times in succession. If from observation we find the rain during thunder-storms has this peculiar characteristic, we may safely consider that rain has something to do with the production of electricity, but as yet our observations are so limited, that it would be unsafe to attempt to form any theory; but we may hope the phenomenon will be observed by others, and also that they will make their conclusions known, and ultimately we may be led to a new meteorological fact.

ON A COMPOUND OF SULPHUROUS ACID AND WATER.

BY M. DÖPPING.

If sulphurous acid gas, previously washed to get rid of sulphuric acid, be passed into a bottle containing distilled water, kept cold by ice, a crystallized substance is formed when a large quantity of sulphurous acid has been absorbed.

At a temperature a little above that of melting ice, these crystals re-dissolve in the surrounding water; but if the bright liquor be exposed to a temperature somewhat below the point of congelation, the crystals are again formed in masses consisting of cubes heaped upon each other.

These crystals may be separated from the liquor at -3°C. , and may be dried in paper preserved in a perfectly dry phial. Between -1° and $-2^{\circ}6$, they begin to moisten, liquefy and eventually disengage sulphurous acid. If an attempt be made to dry by the aid of sulphuric acid under a receiver, at a temperature of -5° , they decompose; their water is gradually attracted by the sulphuric acid, and sulphurous acid is disengaged.

In order to determine the proportion of water and sulphurous acid in these cubic crystals, they were separated from the liquor by a funnel at the temperature -3° to -4°C. ; and after they had been well drained, they were submitted to slight pressure in filtering paper and dried as much as possible. The dried substance was weighed in a closed tube at a temperature below 0°C. , excess of strong solution of chlorine was added to it, and the solution was digested for some time. By means of chloride of barium the quantity of sulphuric acid was determined in the form of sulphate of barytes.

I. 2.157 grms. pressed in filtering paper gave 1.888 of sulphate of barytes.

II. 1.424 grm. of the same crystals yielded 1.330 of sulphate of barytes.

III. 1.625 grm. of these crystals, dried and kept for about 10

days in a corked phial at a temperature of from -3° to -4° , yielded 1.477 of sulphate of barytes.

One hundred parts of these crystals were then formed of—

	I.	II.	III.
Sulphurous Acid	76.02	79.16	76.82
Water	23.98	20.84	23.18

taking as the basis of the calculation the quantity of anhydrous sulphurous acid, corresponding to the sulphate of barytes.

For one equivalent of anhydrous sulphurous acid ($H=1$) 32.15, we have—

	I.	II.	III.
Water	10.14	8.46	9.73

These numbers correspond to 1 equivalent of water = 9.01, so nearly, that no doubt can be entertained of the compound in question being formed of equal equivalents of sulphurous acid and water.

It appears also that there exists another compound of sulphurous acid and water. If the liquor which separates from the hydrate in question, be exposed to a temperature -6° to -7° C., it becomes a crystalline mass which appears to have a lamellar structure. When the temperature approaches 0° , these crystals re-dissolve, and at -2° C. the whole becomes liquid, a phenomenon which is not exhibited by the preceding compound. The author has not yet ascertained the composition of the last described crystals.—*L'Institut*, Octobre 10, 1849.

ON THE METHODS OF ASCERTAINING THE QUANTITY OF BROMINE IN SOLUTION IN MOTHER-WATERS. BY M. FEHLING.

Three methods are adopted for determining the quantity of bromine contained in mother-waters or mineral waters.

1st. The first consists in precipitating by nitrate of silver the chlorine and bromine contained in these liquids, and in treating the mixture of chloride and bromide of silver by chlorine gas, which displaces the bromine.

As the atomic weight of bromine is higher than that of chlorine, the quantity of bromine is readily calculated by multiplying the difference of weight obtained by the coefficient 1.7947, which is merely the equivalent of bromine divided by the difference of the equivalents of bromine and chlorine. This method is not precise except when the liquids contain a notable quantity of bromine.

2nd. The second process proposed by M. Heine, consists in displacing the bromine by æther, and appreciating the quantity according to the intensity of the tint of the ethereal solution.

In operating in this manner on 60 grammes of liquid containing from 0.002 to 0.020 of bromine, and avoiding the influence of the sun's rays, the quantity of bromine set free by the chlorine may be estimated to within about one or two thousandths.

3rd. Lastly, the third process, which is that proposed by M. Fehling, is based on the following fact: when a mixture of an alkaline chloride and bromide is perfectly precipitated by nitrate of silver, the first portions of the precipitate contain all the bromine which the solution contained.

This process, therefore, admits of concentrating the bromine, and employing such a quantity of nitrate of silver as is insufficient to precipitate the whole of the chlorine; and as the object is merely that of obtaining a compound of bromide and chloride very rich in bromide, the first process becomes readily applicable without any risk of serious error.

The silver precipitate ought to be well washed, and in order to decompose it by chlorine, it is to be fused, a quantity being introduced into a tube with a bulb, which is to be heated by a spirit-lamp.—*Journ. de Pharm. et de Chim.*, Septembre, 1849.

DETECTION OF SMALL QUANTITIES OF IODINE.

BY M. L. THOREL.

The method employed by the author for this purpose is the following, and is merely a modified method of using starch. Put into a small vial fifty or sixty grammes of the suspected liquor, or if it be a solid body, diffuse it in a small quantity of water; add six drops of pure nitric acid, and the same quantity of hydrochloric acid; a small piece of paper is then to be covered with a rather liquid preparation of starch and placed at the mouth of the vial, which is to be heated. If the liquor contains iodine, either in the state of iodide or iodate, the paper will assume a violet blue of greater or less intensity. The nitric acid sets the iodine free by decomposing the iodides, if any exist; the effect of the hydrochloric acid is, that it is substituted for the iodine, by decomposing the iodate, if it should be present.

If the paper should not become coloured at the moment of ebullition, the same quantity of the two acids should be added, shaking the vial strongly. In an instant, the spots should appear, and the stratum of iodine will gradually increase. It must not be immediately concluded that no iodine is present if no colour appears, for it is separated with difficulty from certain bodies, as happens with molasses. In such cases a second operation must be performed, adding to the liquor ten to twenty centigrammes of tartrate of potash dissolved in a small proportion of water. Heat is to be applied an instant before the addition of the acids, which on this occasion may be used in the proportion of eight to ten drops of nitric acid, and four drops of hydrochloric acid. After this trial an opinion may be arrived at with great certainty.

By operating in this manner, the presence of iodine may be detected in a mixture which contains only three to four milligrammes. With a mixture of twenty milligrammes of iodine and 200 grammes of salt, very intense spots may be obtained.—*Journ. de Chém. Méd.*, Septembre 1849.

CONTRIBUTIONS TO THE CHEMISTRY OF THE METALS OF
PLATINA. BY M. C. CLAUS.

On examining the residuum of the treatment of platina by its solvent, the author had an opportunity of observing several reactions occurring between the metals of platina and their combinations which had not been remarked, but which appeared to him worthy of attention. The following is a sketch of these reactions.

1. *Chloride of Iridium and Nitrate of Silver.*—It is well known that the chlorine of the solutions of the various platina metals is not precipitated as pure chloride of silver by the nitrate. In employing chloride of iridium, a compound is also obtained, which, according to the author, is an argento-sesquichloride of iridium, insoluble in water or in acids, and difficultly soluble in ammonia, but from which it may always be obtained in the form of rhomboids, as brilliant as diamonds. M. Claus analysed this salt, and found it to correspond to the formula $3\text{Ag Cl} + \text{Ir}^2 \text{Cl}^3$.

2. *Action of Sulphuric Acid and Sulphite of Potash on the Chloride and double compounds of some of the Platina Metals.*—Sulphurous acid reduces the higher chlorides of the platina metals to a lower degree; the chloride of platina into protochloride, and the chloride of iridium to sesquichloride, &c. As to the double salts of these chlorides, sulphite of potash gives with them a series of compounds of peculiar composition which contain sulphurous acid, communicating to them properties which are quite peculiar.

a. *Compounds of Iridium.*—When for the preparation of potassio-sesquichloride of iridium, eight parts of water are poured upon potassio-chloride of iridium in fine powder, and sulphurous acid is passed through the liquor till an olive-coloured solution is formed, the chloride is converted into sesquichloride, accompanied with the formation of sulphurous and hydrochloric acids. This salt, the composition of which is represented by $3\text{K Cl} + \text{Ir}^2 \text{Cl}^3 + 6\text{Aq}$, effloresces readily in warm dry air; it is opaque, and its crystals become covered with a bright green powder. It is insoluble in alcohol, soluble in water, forming an olive-green solution, but by transmitted light it is slightly purple. It has the bitter taste of chloride of iridium; but it is more permanent, and the solution may be evaporated nearly to dryness without decomposition. The alkalis decompose it with difficulty. Aqua regia converts it readily into chloride, and nitrate of silver immediately precipitates, without any blue reaction, the double salt $3\text{Ag Cl} + \text{Ir}^2 \text{Cl}^3$.

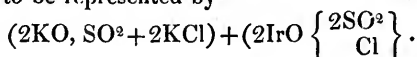
The solution of potassio-chloride of iridium, reduced by sulphurous acid, and from which the greater part of the sesquichloride may be precipitated by carbonate of potash, preserves its olive-green colour at common temperatures; but if it be heated, it passes after some time to a red colour, and eventually to a bright yellow.

It forms also several compounds of peculiar composition, which contain sulphurous acid, and which, when mixed together, may be separated, some in the crystalline form and others in that of powder, by evaporations which are of difficult execution. The author suc-

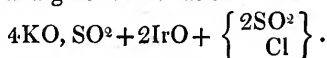
ceeded in isolating three :—1, a rose-coloured crystalline salt ; 2, an amber-coloured substance, having the consistence of Venice turpentine ; 3, a white pulverulent compound. All these compounds contain potash, sulphuric acid, chlorine, and protoxide of iridium in variable proportions. They are nearly insipid, and very slightly soluble in water, disengage sulphurous acid when heated, and are difficultly decomposed by calcination.

Hydrochloric acid dissolves them readily, disengaging part of their sulphurous acid, and converting them into other salts containing an equivalent of chlorine. The aqueous solution gives a white flocculent precipitate with chloride of barium, and the alkalies decompose them with difficulty. They are but slowly oxidized by means of aqua regia ; and before conversion into chloride of iridium, they assume a deep cherry-red colour.

The author has analysed the rose-coloured salt, and finds the composition to be represented by—



The amber-coloured substance resembling turpentine M. Claus has also analysed, and gives as its rational formula—



The white salt of iridium, obtained only in small quantity, has been described and analysed by the author ; its formula is—



On treating this salt with hydrochloric acid, a yellow solution is obtained, which by evaporation yields yellow prisms, considered by M. Claus as a double salt, or sulphite of protoxide of iridium and chloride of potassium, $3\text{KCl} + \text{IrO}^2, \text{SO}^2$.

b. Compounds of Osmium.—The potassio-chloride of osmium undergoes no modification by sulphurous acid at common temperatures ; but when heated, sulphite of potash occasions a partial decomposition, producing a pulverulent precipitate which is a double sulphite of potash and osmium, represented by the formula $3\text{KO}, \text{SO}^2 + \text{Os O}^2, \text{SO}^2 + 5\text{Aq}$. On treating this salt with hydrochloric acid, the double salt, $3\text{K Cl} + \text{Os O}^2, \text{SO}^2$, or chloride of potassium and sulphite of osmium.

c. Compounds of Platina.—The author describes in a few words the preparation of potassio-chloride of platina, which, heated with sulphite of potash, serves for the preparation of a white substance, which the author from his analyses considers as a double sulphite of potash and platina, $3\text{KO}, \text{SO}^2 + \text{Pt O}^2, \text{SO}^2 + 2\frac{1}{2}\text{Aq}$. ; it resembles the salt of osmium, but is more insoluble, almost insipid, heavier, and contains only half the quantity of water. With hydrochloric acid it acts differently from the preceding salts ; for it loses all its sulphurous acid, and is converted into potassio-chloride of platina.

d. Compounds of Ruthenium.—Sulphurous acid acts but little upon the potassio-sesquichloride of ruthenium at common temperatures ; but a solution of the salt treated with sulphite of potash

becomes of a deeper red colour, and a pulverulent isabella-yellow precipitate separates from the liquor. This substance, by repeated solution and crystallization, is obtained of a white colour; and the author is of opinion that it has the same composition as the other salts obtained from the other platina metals; but the small quantity of ruthenium which he had at his disposal prevented him from verifying.—*L'Institut*, Août 1, 1849.

ON THE COMPOSITION OF HONEY. BY M. SOUBEIRAN.

It has been long known that the honey of the bee contains two different sugars, one of which is solid and the other liquid. The former is considered as identical with the granular sugar, which is slowly deposited from the syrup of raisin-sugar, or in that of cane-sugar altered by acids. As to the liquid part of honey, it has been but little studied. M. Biot has, however, stated that it is a sugar which turns the rays of polarized light to the left.

According to M. Soubeiran, honey contains three different sugars, namely, granular sugar, or glucose of chemists; another sugar which rotates to the right, and is alterable by acids; and lastly, a sugar the rotary power of which is exerted to the left, but with an energy which is nearly double that of altered sugar.

M. Soubeiran has found in common honey, sugar which has rotation to the right, and which can be altered; but it is especially abundant in the liquid honey which is contained in the honey-comb. The proportion is so considerable, that a solution of this honey, which had a deviation of $+0.96$ towards the right, acquired, by the action of acids, a contrary rotation equal to -13.78 .

The author describes by the name of liquid sugar of honey, the fluid portion obtained from honey by the use of the press. His experiments were made upon sugar extracted in 1841, and which has been kept ever since unchanged and without any indication of crystallizing. This circumstance is sufficient to distinguish it from altered sugar, which would not have failed to become a solid mass of granular sugar. Liquid sugar of honey possesses, however, a number of characters which belong also to cane-sugar altered by acids; it is, like it, uncrystallizable, and reducible to a sort of barley-sugar, which is transparent and solid, but melts with great readiness. Still further, the liquid sugar of honey is very sensible to the action of alkalies, and is readily destroyed by them. The two sugars have the same chemical composition, and combine with alkalies in the same proportion. This agreement of characters would tend to confound them, were it not that the liquid sugar of honey cannot be converted into granular sugar, and that there is a great difference in their rotary power: this power is almost double in the liquid sugar of honey.

To recapitulate, the experiments contained in this memoir by M. Soubeiran establish the following facts:—Honey is composed of a mixture of three different sugars: one is the granular sugar already known; another is the liquid sugar, which resembles in many par-

ticulars cane-sugar altered by acids, but is distinguished from it in possessing a much stronger rotary power towards the left. The absolute rotary power of liquid sugar of honey at the temperature of 55° F. for the red ray, and at the length of 100 millims., was found to be equal —33·103 towards the left; whilst that of altered sugar under similar circumstances was found to be equal only to —18·933. The liquid sugar of honey retains its rotary power towards the left even after it has been rendered solid; it is one of the few substances which possess this character. The third sugar which constitutes part of honey is distinguished from granular sugar in being unalterable by acids, and from liquid sugar in rotating towards the right. Its proportion is considerable in honey from the comb, diminishes by keeping, and even entirely ceases to exist in solidified honey.—*L'Institut*, Juin 11, 1849.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1849.

Chiswick.—September 1. Heavy rain; lightning, with fine rain 10 P.M. 2. Fine: thunder and lightning, with rain 8 P.M. 3. Very fine: lightning, with rain at night. 4. Fine: cloudy: very clear at night. 5. Hazy: very fine: clear. 6. Clear and very fine. 7. Cloudy: very fine: cloudy. 8, 9. Fine. 10. Cloudy: heavy showers in the evening. 11. Hazy: very fine: clear. 12. Heavy rain. 13. Overcast: rain: clear. 14—16. Fine. 17. Clear and fine. 18. Clear and cold: cloudy. 19. Very fine. 20. Fine. 21. Showery: fine. 22. Fine. 23. Dusky haze. 24. Foggy: very fine: clear. 25. Foggy: exceedingly fine. 26. Foggy: fine: clear. 27. Fine: rain at night. 28. Clear: very fine: overcast. 29. Overcast: fine: heavy rain. 30. Rain.

Mean temperature of the month 57°·76

Mean temperature of Sept. 1848 55°·96

Mean temperature of Sept. for the last twenty-three years 57°·23

Average amount of rain in Sept. 2·73 inches.

Boston.—Sept. 1. Cloudy: rain P.M. 2. Cloudy: rain early A.M., with thunder and lightning. 3. Fine: rain early A.M., with thunder and lightning. 4. Cloudy. 5. Fine. 6. Fine: rain P.M. 7, 8. Cloudy. 9. Fine. 10. Cloudy: rain A.M. and P.M. 11. Cloudy. 12. Cloudy: rain A.M. and P.M. 13. Rain: rain A.M. 14, 15. Cloudy. 16. Fine: rain A.M. 17—20. Fine. 21. Fine: rain and hail early A.M. 22, 23. Fine. 24. Cloudy. 25—27. Fine. 28. Rain: rain A.M. 29, 30. Rain A.M. and P.M.

Applegarth Manse, Dumfries-shire.—Sept. 1. Dull A.M.: cleared: fine harvest day. 2. Hail: thunder: rain A.M.: fine P.M. 3. Dew-like shower A.M.: fine: sultry. 4, 5. Beautiful harvest day: sultry P.M. 6—8. Fine. 9. Showery all day. 10. Very wet A.M.: cleared and fine P.M. 11. Fair A.M.: shower P.M. and thunder. 12. Fair: dull P.M. 13. Fair: high wind. 14. Dull and threatening rain, but cleared and was fine. 15. A shower: looking unsettled. 16. Heavy rain early A.M.: thunder. 17, 18. Fine: very beautiful day. 19, 20. Fine: cloudy: fine harvest day. 21. Fine: cloudy: bar. falling. 22. Fair still. 23—26. Fair. 27. Fair: clear and cold. 28. Rain nearly all day. 29. Rain. 30. Storm of wind and rain.

Mean temperature of the month 58°·5

Mean temperature of Sept. 1848 53°·1

Mean temperature of Sept. for the last twenty-five years . 53°·0

Average amount of rain in Sept. for the last twenty years 3·13 inches.

Sandwick Manse, Orkney.—Sept. 1. Cloudy: drops. 2. Damp. 3, 4. Fog. 5. Bright: cloudy. 6. Cloudy. 7. Clear. 8. Bright: cloudy. 9. Bright: rain: aurora. 10. Bright: clear: aurora. 11. Cloudy: rain: aurora. 12. Rain: drizzle. 13. Rain: clear: aurora. 14. Bright: showers. 15. Cloudy: clear: showers. 16. Showers: aurora. 17. Clear: cloudy. 18. Cloudy: rain. 19. Clear: cloudy. 20. Cloudy: fine: cloudy. 21. Fine: cloudy. 22. Damp: cloudy. 23. Bright: cloudy. 24. Cloudy. 25—27. Bright: cloudy. 28. Cloudy: damp. 29. Bright: cloudy. 30. Bright: drops: clear.

THE
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[THIRD SERIES.]

DECEMBER 1849.

L. *Observations and Experiments on the Noctiluca miliaris, the Animalcular source of the Phosphorescence of the British Seas; together with a few general remarks on the phenomena of Vital Phosphorescence.* By JAMES H. PRING, M.D.*

"A third kind of light arises, no doubt, from living animals which float in the sea, and which must be produced by their peculiar organization, or rather their component parts, which deserve to be better examined by chemical experiment."
—*Tilloch's Magazine*, vol. viii. 1800.

THE occasional phosphorescence of ocean-water has been the subject of observation amongst naturalists from the days of Pliny down to the present time. The phenomenon is peculiar to no sea, and though most brilliant between the tropics, yet it occurs also in the frozen ocean of either pole, and, as may be readily inferred, in every intermediate grade of climate.

Very graphic and highly interesting are the accounts which travellers and others have given of this remarkable appearance; and various are the opinions which have at different times been advanced in explanation of its cause.

It has been conjectured that, during the shining of the sun, light is absorbed by the ocean, and that the extrication of it again renders the water luminous, in a manner analogous to the action exemplified by Canton's pyrophorus, the Bononian stones, &c., or to that which has been termed "insolation."

Again, it has been supposed to depend upon a peculiar electrical state of the atmosphere, or that the ocean itself is at times capable of manifesting this light, as the result of a highly electrical condition of its waters; this last opinion, extraordinary as it may now appear, having enrolled the name of Buffon amongst its supporters.

* Communicated by the Author, having been read before the British Association at Birmingham, September 1849.

Phil. Mag. S. 3. Vol. 35. No. 238. Dec. 1849. 2 D

The attrition of the saline particles against each other, or some unknown combination amongst them, has also been regarded as a source of this marine light, whilst it has further been attributed to the presence of phosphoric matter extricated from decomposing fish, &c.; and lastly, it has been very generally referred, especially in recent times, to a power of phosphorescence possessed by numerous living marine animals, similar, for the most part, in its character to that exhibited by the glow-worm and fire-fly on land.

It is almost needless to observe, that, of the foregoing theories, the last may now be said to be universally admitted as correct; yet it is surprising that even with some of those who have investigated the subject in a scientific point of view, no very distinct ideas seem to be entertained as to the precise nature, even in the instance of our own seas, of the animalcular source to which the light is thus in general terms ascribed; whilst the notions prevalent amongst sailors and others serve only to exhibit how little has as yet been done towards removing the popular ignorance in which the subject still remains enveloped.

Amongst the scientific world again, we find that investigation has chiefly been directed to those instances of vital phosphorescence which are presented by that division of the animal kingdom which is confined to the land; yet this division sinks into comparative insignificance when contrasted with the great variety and infinite multitudes of phosphorescent animals which inhabit the ocean.

With a view, however, to rendering the present notice more complete, we shall glance rapidly at some of the more prominent instances of phosphorescence as displayed by land animals, and then notice briefly some of the more important instances afforded by the tribes inhabiting the sea; dwelling more particularly, as regards the latter, on the example which forms the chief subject of the present communication, the *Noctiluca miliaris*, to which the phosphorescence occasionally witnessed in the British seas is mainly attributable.

If we except the instance of the Great American Bittern amongst birds, which has been stated to possess the power of "emitting a light from its breast equal to that of a common torch, which illuminates the water so as to enable it to discover its prey*," we are not aware that the property of phosphorescence has been attributed to any land animals until we descend so low in the scale as the class of Insects; a division, however, in which this power is very numerously and conspicuously displayed, the family of the Lampyridæ, or glow-worms alone containing about 200 species known to be luminous, whilst

* See Loudon's Magazine of Natural History, vol. ii. p. 200.

the number of the Elateridæ, or fire-flies, possessing the same faculty, amounts to at least thirty. Nor are these the only families endowed with this singular power: it is exhibited likewise by the Scarabæides, and is found also in the *Pausus sphærocerus*, the *Scolopendra electrica*, and in several species of the Fulgoræ or lantern-flies. The ova also of some of these, as in the *Lampyrus splendidula*, are said to be luminous, and the pupa and larva of this insect are reputed to possess the same property, though in a less marked degree. It has been stated also that the common centipede of this country has been observed to be slightly luminous, and the same has been affirmed of the common earth worm; but these statements require further confirmation before they can be received with confidence.

Before quitting this division of our subject, it may be observed that it is usual, in physiological writings, to find reference made under the present head to the instances of luminosity in the living human subject which were brought under the notice of the profession a few years since by Sir Henry Marsh (Prov. Med. Journ. 1842). The instances in question, however, though highly interesting to the pathologist, like the fact remarked by Cabanis of the excess of phosphorus in the brain of maniacs, are nevertheless of a character which must exclude them from our present inquiry.

Passing then from the examples of phosphorescence thus brought rapidly under view, as exhibited by that portion of the animal creation confined to the land, we proceed briefly to notice the instances of the same phenomenon as displayed amongst the tenants of the deep. And here, as led on in the pursuit of this interesting subject to seek the ocean as the field of his further research, the curious inquirer cannot fail to be struck with the vastness and grandeur of the change presented to his contemplation. Here he discovers not only a much greater variety as regards the range and type of animal life amongst which this power of phosphorescence is distributed, but he will recognize some individual instances, which, though so minute as to be revealed only by the aid of the microscope, yet exist in such countless myriads, that the whole element may be said to teem with them.

In order to convey some idea of the general effect of the phosphorescence of the ocean from the presence of a great variety of luminous animals, and as witnessed on a large scale at sea, I shall avail myself of some of the descriptions which have been furnished us by travellers of accurate observation and authenticity. In a highly interesting narrative of a whaling voyage round the globe, from the year 1833 to 1836,

by F. D. Bennett, Surgeon to the Expedition, we find the following account of the phenomenon:—

“During a dark and calm night, with transient squalls of rain, in lat. 43° S. long. 79° W., the sea presented an unusually luminous appearance. While undisturbed, the ocean emitted a faint gleam from its bosom, and when agitated by the passage of the ship flashed forth streams of light which illuminated the sails, and shone in the wake with great intensity. A net, towing alongside, had the appearance of a ball of fire followed by a long and sparkling train; and large fish, as they darted through the water, could be traced by the scintillating lines they left upon its surface*.” And again, “At midnight, on the 1st of December, in lat. 19° N. long. 107° W. (half-way between the group of the Revilla-gigedo and the continent of America), the sea around us presented one uniform milk-white and luminous expanse, as far as the eye could see from the mast-head. It emitted a faint light like that which attends the dawn of day, and bore a near resemblance to a field of snow reflecting the rays of the moon; the horizon being strongly defined, by the contrast of its bright and silver hue with the murky darkness of the sky above. Close to the ship the water appeared brighter than elsewhere, and the dashing of the waves against her bows produced brilliant flashes of light; but it occurred very strangely, that although the waves could be heard lifting in the ordinary manner, it was difficult to perceive them; and the sea appeared as one tranquil unbroken surface. A net and a bucket were employed to ascertain the cause of this phenomenon. The former captured nothing but a few Medusæ of no phosphorescent power; and the water taken up by the bucket, though it was thickly studded by luminous points, contained no tangible bodies.

“A shoal of porpoises came around us at this time; and as they sported in the luminous ocean, darting rapidly beneath the surface, their dark bodies enveloped, as it were, in liquid fire, they tended to complete a scene, which, if correctly pictured, would appear rather as the fiction of a fairy tale than the effect of natural causes†.”

In a small work entitled ‘The Ocean,’ by Mr. P. H. Gosse, we have also an interesting notice of the same appearance, which is thus described:—“The most usual appearances, as far as they have fallen under my own observation in the Atlantic, are as follow:—On looking over the stern, when the ship has steerage-way, her track is visible by a line or belt of light, not a bright glare, but a soft, subdued, yellowish light,

* Pp. 17–18, vol. i.

† Pp. 289, 290.

which immediately under the eye resembles milk, or looks as though the keel stirred up a sediment of chalk which diffuses itself in opaque clouds through the neighbouring water, only that it is light and not whiteness. Scattered about this cloudiness, and particularly where the water whirls and eddies with the motion of the rudder, are seen innumerable sparks of light distinctly traced above the mass by their brilliancy, some of which vanish and others appear, while others seem to remain visible for some time. Generally speaking, both these phenomena are excited by the action of the vessel through the waves, though a few sparks may be observed on the surface of the waves around. But now and then, when a short sea is running without breaking waves, there are seen broad flashes of light from the surface of a wave, coming and going like sudden fitful flashes of lightning. These may be traced as far as the eye can reach, and in their intermittent gleams are very beautiful; they have no connexion with the motion of the ship*."

When we inquire more precisely into the particular sources of this marine light, we find it distributed, as before mentioned, far more extensively amongst the various grades of animal life in the ocean, than amongst those of the land. Although the fact has been somewhat called in question, and the light attributed to the disturbance of the surrounding luminous water, yet there appears little doubt that the power of phosphorescence is actually possessed by animals ranking as high as the class of fishes. Thus in the narrative of Mr. F. D. Bennett, above alluded to, after referring the general luminosity of the ocean on a particular occasion to the presence of *Medusæ*, he proceeds: "Though the discovery of these *Medusæ* was a satisfactory explanation of the phosphorescent appearance of the water, I had yet to learn that the latter effect was partly produced by living, bony, and perfectly organized fish: such fish were numerous in the sea this night; and a tow-net captured ten of them in the space of a few hours. They were a species of *Scopelus*, three inches in length, covered with scales of a steel-gray colour, and the fins spotted with gray. Each side of the margin of the abdomen was occupied by a single row of small and circular depressions of the same metallic-gray hue as the scales; a few similar depressions being scattered also on the sides, but with less regularity. The examples we obtained were alive when taken from the net, and swam about actively upon being placed in a vessel of seawater. When handled or swimming, they emitted a vivid phosphorescent light from the scales, or plates, covering the body and head, as well as from the circular depressions on the

abdomen and sides, and which presented the appearance of as many small stars spangling the surface of the skin. The luminous gleam (which had sometimes an intermittent or twinkling character, and at others shone steadily for several minutes together) *entirely disappeared after the death of the fish.*" It is almost needless to observe that there appears little room for questioning a fact thus minutely and accurately described. Another instance belonging to the Shark tribe, (the *Squalus fulgens*) has also formed the subject of minute investigation by the same observer. After describing the form and structure of this fish, and noting accurately the portions of the body devoted to the production of the light, he remarks, "I am inclined to believe that the luminous power of this shark resides in a peculiar secretion from the skin. It was my first impression that the fish had accidentally contracted some phosphorescent matter from the sea, or from the net in which it was captured; but the most rigid investigation did not confirm this suspicion; while the uniformity with which the luminous gleam occupied certain portions of the body and fins, its permanence during life, and decline and cessation upon the approach and occurrence of death, did not leave a doubt in my mind but that it was a vital principle, essential to the œconomy of the animal."

Many of the Crustacea, of which the *Cancer fulgens* and the *Oniscus fulgens* may serve as examples, are universally admitted to be highly luminous. The Mollusca, both testaceous and naked, afford well-marked instances of phosphorescence. Amongst the former, examples of which are somewhat rare, may be noticed the *Cleodora cuspidata*, described more particularly by Mr. Bennett, and also some of the Pholades, &c.; whilst the latter contain the Salpæ, the Pyrosomata, &c. Amongst the Annelida we find the *Nereides* and the *Polynoë fulgurans* enjoying the same faculty; and the same may be observed very generally of the Acalephæ, the general phosphorescence of the ocean being chiefly due to the numerous kinds of Medusæ, Polypiferæ, Rotiferæ, and Infusoria included under this class, and more particularly in our own seas to the microscopic example it contains, the *Noctiluca miliaris*, which brings us to the special object of the present communication.

In noticing more particularly this minute yet powerful source of oceanic light, I shall first give a brief account of the general appearance imparted by its presence to the waters of our bay, as exhibited for several successive nights during the months of July and August last, and then describe more particularly the little animal itself, and the various experiments to which it has been subjected.

Taking, as a good example, the night of the 9th of August last, which was remarkably bright and cloudless, and at the same time serene and mild, the phosphorescence of the waters of our bay, which had been visible in a less degree for many preceding nights, assumed about midnight a very brilliant and beautiful appearance. Seen from a distance, the aspect presented at this time by the whole surface of the water was that of a diffused silvery light, which caused the dark headland of Brean Down, on the opposite side of the bay, to appear as if laved by liquid silver. On approaching nearer the water's edge, so as to observe more distinctly the waves as they broke gently and with even regularity upon the strand, the effect can only be compared to masses of liquid fire rolling and pouring in, and diffusing itself along the shore. On the north-west point of the bay is a projecting rock called Knightstone, joined by a raised causeway to the main land, and forming at high water a sort of short pier, alongside of which a few small vessels and boats usually lie at anchor. The shelter against the current, together with the shade afforded by the rock and walls of the pier, combined to prove highly favourable to an observation of the effect in comparatively still water, and under increased darkness. On looking down then at this point, between the sides of the vessels and the rock, a constant scintillation was to be observed on the surface of the water, very similar to a shower of fire or sparks, the luminous points, some larger, others less in size, perpetually dancing up as it were to the surface, and soon again disappearing to be replaced by others. The larger points, some of which were as large as a pea, were produced, I conceive, from several of the little animals having floated into close approximation or contact, whilst the smaller ones, I imagine, proceeded from detached individuals.

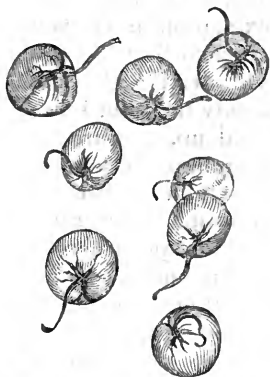
On striking the surface of the water lightly with an oar, not only the spot immediately touched, but also all the water dashed up, appeared, whilst raised in the air, and again on falling into the surrounding water, to bear an exact resemblance to liquid fire, affording a spectacle of the most brilliant description. Several bottles were then filled with the water from this spot where it appeared brightest. I likewise procured a bucketful of the same kind, and the whole was brought home for the purposes of observation and experiment. As tending to convey some further idea of the luminous power of the water in its natural condition, it may be mentioned that on rendering my room perfectly dark and agitating the water in one of these bottles, it perfectly illuminated any object in its vicinity, so as to enable me to distinguish various

objects on my table, pictures hanging against the wall, &c.; the luminous effect, however, being very transitory, and only at the moment of agitation.

Viewed the following morning by daylight, innumerable very minute gelatinous bodies, of a globular form, could be perceived even with the naked eye, floating near the surface of the water; and on gently shaking the bottle containing them, they could be observed to descend to a short depth, gradually, however, rising again to their former level when left at rest. From repeated observation of this fact, it would appear that these little animals are naturally, or otherwise possess the power of rendering themselves, specifically lighter than sea water; and this property appears to be a living attribute, since it ceases, and they are seen immediately to sink to the bottom of the vessel, upon the occurrence of death.

Placed under the field of a microscope, and magnified to about the size of a large pea, the *Noctiluca* presented the appearance of a highly delicate and translucent membranous sac, of almost spherical form, and having the look on one side as if the mouth of the sac had been puckered up and turned inwards; yet this puckered part does not take an exactly circular form corresponding to a description which has been given of it, as "producing such an appearance as would arise from tying the neck of a round bag and turning it into the water;" but extends rather in a longitudinal direction, giving somewhat the effect of a longitudinal cleft; and from one end of this cleft is observed to protrude a lengthened filamentous body, which seems to be a sort of tentaculum, and during the life of the animal is seen to be in almost constant motion.

I have endeavoured in vain to obtain a more accurate view of this appendage; but the state of constant motion it is in, together with the extreme minuteness and delicacy of the whole animal, have hitherto rendered my efforts unavailing. That it is occasionally employed as an organ of locomotion there can be no question, and some of the movements executed by it have appeared to me very surprising. Thus I have witnessed it extended above the animal, and then used as a fulcrum, as it were, to draw the body of the animal upwards towards itself; yet



Magnified view of the *Noctiluca miliaris*. Natural size, $\frac{1}{1000}$ th part of an inch in diameter.

by what power so fine and hair-like a member can be made to oppose such resistance to the water, as that the comparatively large globular mass composing the body of the animal should be drawn to it, rather than that it should pass to the globe, I am unable to determine with precision, and consequently refrain from offering a merely conjectural opinion.

The method of examination which I have found to be the most convenient, and from which the foregoing description is taken, is to pour a small quantity of the luminous water into a watch-glass and then submit it to the microscope, by which means the little animals still remain floating in the water, and their movements, under the eye of the observer, are in no way interfered with. Examined in this manner, there is nothing to be discovered to indicate any special luminous organ, or the precise part of the animal devoted to the production of the light; but in several specimens I could clearly observe a mass of loose flocculent mucus adhering to the part which has been described as being puckered in, and more immediately near the insertion of the tentaculum; so that I am disposed to believe that the phosphorescent principle resides in this mucus, and is probably most vivid at the moment of its secretion, the secretion itself appearing to be influenced and thrown out more abundantly under circumstances indicating danger, serving thus to account for the brilliancy with which the light is manifested on first agitating the water after it has been allowed to remain some time at rest. It seems probable, also, that the motion of the tentaculum may at times contribute somewhat to the effect, by disturbing the mucus, and thus bringing a newly-exposed surface of it into contact with the water; the occasional scintillations to be witnessed, even where the water is under circumstances of perfect repose, being, in all probability, thus produced.

The extreme minuteness and delicacy of this little animal (its natural size being stated not to exceed the $\frac{1}{1000}$ th part of an inch in diameter), have no doubt been the causes that have interfered to prevent its attracting any great share of popular attention; and which have also occasioned its being frequently overlooked, as formerly stated, even by scientific observers. Thus, in the passage formerly quoted from the work of Mr. F. D. Bennett, he mentions that a bucket of water which had been taken up, "though thickly studded with luminous points, contained no tangible bodies;" there can be little doubt that these "luminous points" were in reality due to the presence of these minute Noctilucae; and the same remark may also be extended to a similar passage which occurs at p. 321, vol. ii. of the same work. In some cases, on the other hand, it would

appear that the same animal has been alluded to, but under a different title. Thus, although, it must be confessed, imperfect in its details, no one can read the description given by Macartney of the *Medusa scintillans*, without recognizing its full applicability, as far as it goes, to the subject of the present notice. Two other instances, the *Medusa hemisphærica* and the *Beroë fulgens*, are also described by Macartney as occasionally to be met with in the British seas; but their comparative rarity has led this author himself to regard the minuter example, of which we are now treating, as "the most frequent source of the light of the sea around this country," and even also "in other parts of the world." Passing, then, from these general considerations as regards the animal itself, we shall proceed to detail the various experiments to which it has been subjected, which may be most conveniently treated of in the following order, viz.—1, experiments subjecting the luminous water to the action of galvanism; 2, to the action of various gases; 3, to the action of the strong mineral acids; 4, to the action of æther and chloroform, &c.

1. *Effects of Galvanism and Electro-magnetism*.—Subjected to a simple galvanic current from two of Smee's batteries, no very perceptible effect could be observed to be produced. I then attached an electro-magnetic coil to the batteries, and thus passed the electro-magnetic current through the water for some time: at first no very appreciable result appeared to follow; but in a short time a steady and continued glow of light was given out from the whole of the water, the surface of which appeared further as if spangled with numberless minute but persistent points of light. After a short time the light began to grow more faint, and in a quarter of an hour had ceased altogether, without the possibility of its being reproduced, the loss of the light being evidently dependent upon the death of the animalculæ.

2. *Gases. Effects of Oxygen*.—On filling a bottle with oxygen gas, and allowing some of the gas to escape so as to be replaced by a portion of the luminous sea-water, the phosphorescence of the Noctiluca contained in the latter could be perceived to be sensibly increased when the water was agitated with the oxygen, but no continuous or persistent glow of light followed this experiment. For upwards of a week, however, the little animals continued to live beneath this atmosphere of oxygen, evidently emitting, on agitation, for several successive nights during which the observations were continued, the same amount of increased light as had been observed to occur in the first instance.

Effects of Nitrogen.—Subjected to the influence of nitrogen,

in the same way as has just been described with respect to oxygen, the results were much less marked than might have been anticipated. The *Noctiluca* confined beneath the nitrogen continued to live, and to display a vivid phosphorescence when the bottle was agitated for above the space of a week after the experiment was first instituted. If any difference could be observed between this experiment and the former, it was that the brilliancy of the light was somewhat less in this than in the former, being probably about equal to what it would have been if atmospheric air had been employed in place of nitrogen.

Effects of Nitrous Oxide Gas.—On being submitted in the same manner to the action of this gas, the *Noctiluca* appeared to be no otherwise affected than under a similar employment of atmospheric air. They were alive and phosphorescent at the end of ten days from the commencement of the experiment. The intensity of the phosphorescence, however, appeared to be neither augmented nor diminished by the action of this gas.

Effects of Sulphuretted Hydrogen Gas.—On treating a portion of the luminous water with this gas, the phosphorescence was instantly destroyed, all the *Noctiluca* being immediately killed; thus further demonstrating and establishing the well-known power of destructiveness to animal life which characterizes this gas.

Effects of Carbonic Acid Gas.—Of all the gases hitherto noticed, the carbonic acid is the most remarkable in its effects on the luminous sea water. Having filled a bottle with this gas, and introduced it under the water so as to allow a portion of the gas to escape and be replaced by the water, in the same manner as in the other instances just related, the luminous property of the water was not only brought out and highly increased, but was rendered permanent for at least a quarter of an hour, during which time the effect might be compared to a bright incandescent glow, of sufficient intensity to render the bottle visible from a distant part of the room; and when approached more nearly, to enable me to discern the hands of a watch by the sole aid of the light thus afforded. At about the expiration of fifteen minutes the light became gradually fainter, and in about twenty or twenty-five minutes had totally ceased; the darkness, as in the other cases, being evidently connected with the death of the animalculæ, which on being brought into the light, could be seen lying at the bottom of the vessel.

A second bottle of this gas was then procured and employed in the same way, for the purpose of ascertaining the effect of

the admission of atmospheric air, at the time when the phosphorescent glow was beginning to grow faint. The result, however, did not appear to be in any way influenced, nor was the light in any degree resuscitated by this variation of the experiment.

Effects of Hydrogen.—Submitted to the action of this gas, no very marked effect appeared to be produced upon the water. On agitating a portion of it in a bottle containing this gas, numerous specks of light, indicating the presence of the animalculæ, could be perceived for many successive nights; but the light appeared somewhat less vivid in its character than that afforded with atmospheric air, or in the instance before mentioned, in which pure nitrogen was employed.

Effects of Atmospheric Air.—The influence of atmospheric air is here introduced merely as affording a standard of comparison with the effects obtained from the employment of the gases. A bottle of the luminous water, procured at the same time as that used in the preceding experiments, retained its luminosity a few days longer when subjected only to atmospheric air, than under the employment of any of the above-mentioned gases; but the phosphorescence was only to be observed on the occurrence of any agitation of the water. It appears also that the luminous property is retained longer when the vessel or bottle containing the water is kept closed or corked, than when it is left entirely open and freely exposed to the action of the air.

3. *Effects of the strong Mineral Acids.*—For the purpose of ascertaining whether the phosphorescence would be in any way affected by the strong mineral acids, a small quantity of water was placed in each of three glass vessels, and then a few drops of acid were added to the water in each vessel, this latter part of the experiment being of course conducted in the dark. On letting fall a few drops of strong *sulphuric acid* into the water, the latter immediately emitted a bright light which remained for the space of a minute or two, after which it almost immediately disappeared.

The effect of strong *nitric acid* appeared in no way to differ from that produced by the sulphuric acid; but treated with strong *hydrochloric acid* in the same manner, the increased luminosity of the water was much less conspicuous than in either of the former instances, and the darkness ensued almost instantaneously.

Effects of Æther and Chloroform.—A few drops of æther dropped into the sea-water in the dark appeared instantly to deprive it of its luminous property, no degree of agitation to which it was subjected being found capable of eliciting the

smallest scintillation after the addition of the æther. On substituting chloroform, however, in the second experiment in the place of the æther, a very bright and persistent phosphorescence was given out for the space of a few minutes, after which the water speedily became dark, the animalculæ being evidently killed. Before taking leave of this division of the subject, it may be as well to notice the influence simply of fresh water upon the Noctiluca. On pouring some of the sea-water, rendered luminous by the presence of these little animals, into a vessel already containing some fresh rain-water, a subdued continuous glow was given out from several luminous points for a short period, during which the specks of light were seen to subside to the bottom of the vessel, and very speedily afterwards to become totally extinguished.

Of the foregoing experiments, those connected with the employment of the gases appear to be the most interesting; and any degree of importance they may possess will be best appreciated from their bearing upon the topics now to be brought under notice, in the form of a few concluding remarks of a general character on the

Phænomena of Vital Phosphorescence.—The development of light as the result of a vital function, and as constituting an essential feature in the œconomy of some forms of animal life, is a phænomenon of so interesting and remarkable a nature, that it could not fail to arrest the attention of naturalists and philosophers in almost every age. It is, however, only since the impulse which the cultivation of chemical science received about the latter part of the last century, that the subject has come to be investigated in the true spirit of scientific inquiry. It was about the period here alluded to, also, that the animalcular source of the phosphorescence of the sea was first indisputably established; and some valuable papers appeared on the subject in the excellent journal conducted by Mr. Tilloch. At this time, however, no higher object appears to have been sought, than the mere establishment of the fact, as just stated, of the dependence of the phosphorescence of the sea upon an animalcular origin. Many individual instances of phosphorescent marine animals were, indeed, adduced in support of the new doctrine; but this was done without much method or accuracy of detail; and the minuteness and transparency of the little animal which forms the subject of the present notice appear to have occasioned its being at that time overlooked.

At a period, then, when the fact of the animalcular source of marine phosphorescence was barely admitted, and may be said to have been almost a question still *sub judice*, it was scarcely

to be expected that those forms of animal life, whose very existence was held to be problematical, should themselves be made the subjects of actual experiment; and it is accordingly among those more undoubted and easily accessible examples afforded by the insects to be found on the land, that the study of the phænomena of vital phosphorescence has been chiefly prosecuted; the glow-worm and the fire-fly having been generally selected for the purposes of experimental investigation, from the period in question even down to the present time.

Thus, in the second edition of Chaptal's Chemistry, published so long since as the year 1795, we read that "Mr. Forster of Göttingen found that the light of glow-worms is so beautiful and bright in oxygenous gas, that one single insect was sufficient to afford light to read the *Annonces Savantes* of Göttingen, printed in very small character." The same fact is likewise referred to, and ably commented on, in an excellent paper on the phosphorescence of the Luciole (*Lampyrus Italica*) by Dr. Carradori, in the second volume of Tilloch's Magazine; and similar notices are thus incidentally to be met with interspersed throughout the mass of physiological and chemical writings with which science is now enriched. It is, however, to Professor Matteucci that we are indebted for the most accurate and comprehensive experiments that have hitherto been undertaken in connexion with this department of inquiry; and although still confined principally to the glow-worm, his observations may fairly be assumed to afford the best exposition of the existing state of knowledge on the subject of phosphorescence now extant; and I must accordingly refer to his valuable lectures, as published by Pereira, all those who may be desirous of becoming acquainted with the minute structure of the phosphorescent organs of the glow-worm, and such other topics as do not appear to fall strictly within the design of the present paper.

On comparing the results obtained from the action of various gases on the Noctiluca, as described in a former part of this communication, with the effects produced by the same agents upon the glow-worm as recorded by Professor Matteucci, some very remarkable differences will be found to present themselves to our observation. It is right, however, to bear in mind the different circumstances under which the experiments are necessarily conducted in the two instances; the animalculæ in the former, being brought into contact with the gas only through the medium of the water in which they float; whereas in the case of the glow-worm, the insect is freely and entirely exposed to the gas in which it is immersed.

Bearing in mind, then, and making suitable allowance for

this difference of circumstance in the two instances, we shall proceed to note some of the more remarkable points of contrast which they present to us, and which may be thus summarily enumerated.

As in the instances recorded by Matteucci and other observers, with respect to the glow-worm, so with regard to the *Noctiluca* under consideration, the phosphorescence was found to be remarkably increased by oxygen; in the former, however, not only was the light increased in brilliancy, but also in duration; whereas we have seen that the *Noctiluca*, confined under an atmosphere of oxygen, died somewhat sooner than those confined in a bottle containing atmospheric air.

In hydrogen, glow-worms are found to lose their phosphorescence at furthest in about twenty-five or thirty minutes; whilst the *Noctiluca*, under an atmosphere of this gas, continued to emit scintillations at the end of eight or nine days from the commencement of the experiment. But it is in respect to carbonic acid gas that the most remarkable contrast is exhibited in the two cases. On placing glow-worms in this gas, Prof. Matteucci found that in a few minutes the light entirely disappeared; whereas, in the case of the *Noctiluca*, we have seen that there is no agent which has the effect of increasing the brilliancy of the light so powerfully as this gas, at the same time that the bright phosphorescent glow formerly described is rendered permanent for the space of fifteen or twenty minutes. After the lapse of this time, however, this gas proves as fatal to the *Noctiluca*, as to the glow-worm; and to the former, without the power exhibited by the latter, of resuscitation of the light by the admission of atmospheric air.

The effects of sulphuretted hydrogen gas appear to be precisely the same on the glow-worm and on the *Noctiluca*, both being very speedily destroyed by it.

Although those which have now been cited appear to be the only instances which offer a fair opportunity for direct comparison, yet there are several other points which seem naturally to demand notice in this place in connection with the experiments formerly detailed. The phenomenon of vital phosphorescence has been regarded as presenting an analogy to the function of respiration, if not connected with it. Thus, in a paper in *Tilloch's Magazine* (vol. x) on the Phosphorescence of Ocean-water by Prof. Mitchell of New York, we find the maintenance of the process of phosphorescence ascribed to the presence of a supply of oxygen as conveyed by the arterial blood, the process in fact being compared to respiration as expressed in the following somewhat curious passage:—"The light, then," says he, "which these marine ani-

mals (the larger Medusæ) exhibit, may be concluded to be produced by a function in them analogous to the respiration of animals which are of larger size and more complicated structure. The only reason why it is visible from their bodies is, that the gelatinous matter of which they consist is transparent. It is not improbable that the same phænomena would be as obvious in the bodies of other creatures, and even of human beings, if the opacity of the materials of which we consist did not hinder the light within us from shining so as to be seen."

From the opinion expressed by Matteucci, he would appear to compare the process of vital phosphorescence to a species of combustion, in which however he also recognizes the necessity for the presence of oxygen. In commenting on the various experiments to which he subjected the phosphorescent matter, he observes, "From all these experiments, I conclude that carbonic acid is produced by the contact with oxygen of the phosphorescent matter alone, separated from the rest of the animal; that the light ceases to be produced when this gas is not present, and that by the contact of the latter, light and a volume of carbonic acid, equal to that of the oxygen consumed, are produced; and that the phosphorescent substance of this insect, when not luminous, does not act on oxygen. It is therefore natural to suppose that the luminous segments of these animals, being enveloped by transparent membranes, and by means of the numerous tracheæ discovered here and there in these animals, atmospheric oxygen is brought in contact with a substance, *sui generis*, principally composed of carbon, hydrogen, oxygen and azote." And again, "The example of an organic substance which burns in the air by absorbing oxygen and emitting carbonic acid, is not new; this is the case with decaying wood, with oiled cotton, with finely pulverized charcoal, and with many other substances liable to spontaneous combustion."

On this question, however, there appears to be some ground for a difference of opinion. The experiment in which the *Noc-tilucæ* continued for many days to emit very vivid scintillations when confined beneath an atmosphere of nitrogen, must be held to militate against the preceding explanation. It is asserted also by Carradori, that the *Luciole* will shine in a barometrical vacuum, but the experiments of Matteucci on the same point limit this effect to two or three minutes. Admitting therefore the correctness of the latter observation, it may still be questioned whether the effect, even for the time here specified, can be wholly ascribed to the presence of oxygen. Again, when operating on the phosphorescent matter only of

the glow-worm, this matter being separated from the entire insect, it was found by Matteucci to retain its luminous property for thirty or forty minutes after it was placed in pure hydrogen or carbonic acid gas. Here then we have the phosphorescence continued much longer than in the case of the barometric vacuum; and where, it may be asked, was the supply of oxygen that maintained the combustion above half an hour in this instance, or how can this fact be reconciled with the statement formerly cited, "that the light ceases to be produced when this gas (oxygen) is not present?" In reference to the same point, I may here advert to the *increase* of light from the effect of carbonic acid on the *Noctiluca* formerly described, and may quote also an experiment from the observations of Macartney, in which he states, "Some of the scintillating and hemispherical species of *Medusa*, contained in a small glass jar, were introduced into the receiver of an air-pump, and the air being exhausted, they shone as usual when shaken; if any difference could be perceived, the light was more easily excited, and continued longer in vacuum*."

A consideration of these and some similar facts would tend rather to the conclusion drawn by Carradori in reference to the experiments of Forster formerly quoted, on which it is remarked, in the 2nd vol. of Tilloch's Magazine, "The observation made by Forster, that the *Luciole* diffused a more vivid light in oxygen gas than in atmospheric air, does not, according to Carradori, depend upon a combustion more animated by the inspiration of this gas, but on the animals feeling themselves, while in this gas, in a better condition;"—a conclusion, which seems to furnish a view of the question of sufficient importance, to say the least of it, to merit some notice. On the other hand, the instance of the detached luminous segments placed under oxygen, and appearing to absorb a portion of the gas, lends weight to the opinion of Matteucci. It appears, however, by no means certain, even in this case, that the oxygen found to be absorbed, had actually entered into combination with those particles only which are immediately concerned in the production of light; it may, on the contrary, be supposed to have been absorbed also, if not principally, by the other constituents of the organic matter with which the immediate light-emitting particles are combined. And this brings us now to the consideration in the next place of the

Chemical Nature of the Phosphorescent Matter.—In the earlier inquiries respecting the proximate cause of vital phosphorescence, we find that the actual presence of phosphorus

* Phil. Trans. 1810, part 1. p. 285.

in some form of combination was deemed necessary to the display of the luminous property, and in fact that this substance was regarded as the essential ingredient entering into the composition of the phosphorescent matter. Thus it is stated by Carradori, "Behind this receptacle is placed the phosphorus, which resembles a paste having the smell of garlic, and very little taste*." Again, we find Chaptal speaking of it without any hesitation as a phosphoric oil. After noticing the solubility of phosphorus in oils, and more especially in the volatile oils, he observes, "The combination of phosphorus and oil appears to exist naturally in the glow-worm, *Lampyrus splendidula*, Linnæi. Forster of Göttingen observes that the shining matter of the glow-worm is liquid. If the glow-worm be crushed between the fingers, the phosphorescence remains on the finger†."

More recently, again, we find Müller enumerating several facts bearing on the subject, and deducing from them the following conclusion:—"From all the above facts, the opinion of Treviranus appears most probable; namely, that the light is derived from a matter containing phosphorus, which is formed under the influence of light, but, once formed, is in some measure independent of light‡." Nor am I indeed aware that this notion of the actual presence of phosphorus in the phosphorescent matter of living beings, has ever, previously to the observations of Matteucci, been entirely rejected.

After detailing the influence of the various reagents employed in an elaborate chemical examination of the phosphorescent matter, Matteucci observes respecting it,—“It does not present any obvious trace of phosphorus; of this fact I have assured myself by calcining this matter several times in a platinum crucible, and by treating the dissolved residue with the tests which indicate the presence of the phosphates. From all we have now stated, we can no longer regard the presence of phosphorus as the cause of the light in these insects§;” and in another place, as before mentioned, it is said to be “a substance, *sui generis*, principally composed of carbon, hydrogen, oxygen and azote.”

On a point like the present, however, where the statement just mentioned is so entirely in opposition to all previous observation, it would seem desirable that we should know with certainty what is the smallest possible amount of phosphorus which is capable, when placed under the most favourable cir-

* Tilloch's Magazine, vol. ii. p. 79.

† Elements of Chemistry, vol. iii. p. 362.

‡ Elements of Physiology, vol. i. p. 103.

cumstances of combination, of giving rise to the phænomena of phosphorescence. The experiments on which Matteucci grounds his opinion, were no doubt very carefully conducted; yet it may be questioned whether a different result might not be obtained, could a test of such extreme delicacy be rendered applicable in this instance, as we are furnished with by Marsh's apparatus in respect to arsenic. Whilst still upon this subject, I may mention that experimenting some years since with a view to test the theory of the luminous matter of the glow-worm being a natural phosphoric oil, and at the same time to imitate artificially the experiment of Forster, I enclosed some phosphoric oil in a delicate membranous sac, which I then introduced into oxygen gas. The result however was anything but favourable to Chaptal's theory, since the phosphorescence, which had been well-marked whilst in atmospheric air, ceased immediately upon the immersion of the bladder in oxygen.

It has been supposed by many experimenters that vital phosphorescence is remotely connected with, or dependent upon, the action which is termed "insolation"; and although we find experiments detailed by Matteucci which at first inclined him to this opinion, yet, on a more careful and exact repetition of them, he was led to admit that "when the insect is placed in its natural conditions with regard to temperature, humidity, &c., and continues to be nourished, the phosphorescent matter is preserved independent of solar action," a conclusion with which the result of the observations made by myself, in the case of the *Noctiluca*, entirely corresponds.

In reverting here to the experiments on the *Noctiluca*, it may be observed that the medium in which they live gave an opportunity for certain experiments which cannot be obtained in the case of the glow-worm—I refer more especially to the influence of galvanism and electro-magnetism formerly described. From the agency of the former however we have seen little or no effect to be produced; whilst the influence of the latter appeared to be only of the nature of a powerful stimulus, not dissimilar in its character from that produced by the strong mineral acids.

An experiment may also be mentioned here, a notice of which, from its negative character, was omitted in its proper place; and is now briefly introduced lest it should be supposed, from the omission, to have been overlooked—I allude to the effect of temperature on the luminous sea-water. On placing a bottle of the sea-water in a vessel containing common spring water at the temperature of 90° , no remarkable effect or increase of light was to be observed; and it has been

remarked, on the other hand, that when the sea-water is converted into ice it still retains its luminous property.

It now remains only to offer a few words with respect to the

Use of vital phosphorescence, a subject, which, as it has already proved the fertile source of a great amount of speculation, more remarkable for its ingenuity than for any more satisfactory result, it will be my endeavour to dismiss without increasing the accumulated mass of conjecture with which it is already encumbered. As the instances of vital phosphorescence occur amongst so varied and wide a range of the animal creation, it is only reasonable to infer, that in different individual instances this faculty will serve respectively a different purpose. Thus in the highly predaceous luminous shark, the *Squalus fulgens*, formerly noticed, it may readily be assumed with Mr. Bennett*, "That the phosphorescent power it possesses is of use to attract its prey, upon the same principle as the Polynesian islanders and others employ torches in night fishing." In the insect tribe, again, it has no doubt been correctly described as furnishing "à la lettre, le flambeau de l'amour."

Amongst the lower marine tribes, however, the object of this luminous provision is generally admitted to be much less apparent; and the most probable conjectures that have been formed respecting it, are those by which it is regarded as an engine of defence. Mr. Kirby appears to have viewed it altogether in the latter light; upon which Mr. Bennett has the following remark:—"I cannot believe, with Mr. Kirby, that it serves as a mode of defence; because from what we know of the nature of fishes, this refulgence would be one of the surest means of bringing their probable enemies upon them; and if we are to regard the œconomy in a destructive point of view, we might rather suppose that it is intended to direct the nocturnal predaceous fishes to their food. * * * But it would be unjust to accuse Nature of thus wantonly investing her creatures with a charm that can only tend to their destruction†." The train of argument here employed may be cited as an example of the uncertainty and perhaps error into which the mind may be betrayed, when it quits the plain and sober path of reason, to wander in the field of mere conjecture.

In the instance of the *Noctiluca*, as already hinted, and for the reasons formerly assigned, it seems probable that this luminous property serves the purpose of defence; though whether this is its sole object, is a question for future investigation to determine.

* Vol. ii. p. 258.

† Vol. ii. p. 325.

Whilst then we have thus cursorily glanced at some of the theories which have been advanced on this subject, we are compelled to confess that even those best established amongst them are far from being in any degree satisfactory. In the instance of the glow-worm, for example, the purpose which this provision fulfills may be said to be universally admitted and agreed upon; yet what sufficient reason can be assigned for such a faculty being conferred almost exclusively on this insect, in preference, as it were, to all others? On this head one cannot do otherwise than express an entire concurrence in the conclusion of Matteucci, that this phænomenon, "in its exceptional character, is one of those mysterious singularities which Nature seems to have distributed amidst the immense variety of beings, almost without any previous attention to the animals on which she bestows them, as if merely for the purpose of constraining us to admire with humility the power of her creative skill."

In concluding this brief and imperfect sketch of a subject involving so many points of interest, my object will have been attained, if what has been here advanced should serve the purpose of rendering somewhat more defined the outline of a picture which yet remains to be filled up in its details, and still invites the hand of the more skilful artist to the undertaking. The subject of vital phosphorescence, as embracing a minute and accurate account of all the known examples of the phænomenon, its causes, its uses, and a yet more critical inquiry into the chemical nature of the phosphorescent process than it has hitherto received, is still open to investigation. And lest any should be inclined to underrate this humble yet interesting department of inquiry, I would venture, in reference to it, to bring to their recollection the high authority of Bacon, who says, "And here men ought to lower their contemplations a little, and inquire into the properties common to all lucid bodies; as this relates to the form of light; how immensely soever the bodies concerned may differ in dignity, as the sun does from rotten wood, or putrefied fish*."

Weston-super-Mare, Sept. 8, 1849.

* On the Doctrine of the Human Soul, p. 113. vol. i., Bacon's Philosophical Works.

LI. *On the Vibratory Movements which Magnetic and Non-magnetic Bodies experience under the influence of external and transmitted Electric Currents.* By Professor DE LA RIVE*.

M. WERTHEIM has published in the *Annales de Chimie et de Physique*, 3rd series, vol. xxiii. p. 302, some further researches on the sounds produced by the electric current. He directs attention to the fact that these sounds are of two kinds; those proceeding from the action of an external current which magnetizes an iron bar or wire, and those produced by a current transmitted either through an iron bar or wire. The sounds of the first kind were discovered as early as 1837 by Mr. Page, and afterwards investigated by MM. Marian, Matteucci, Wartmann† and myself. I was the first who indicated the existence of the sounds of the second kind, which Mr. Beatson discovered nearly at the same time as myself, and which have also been the object of the researches of the physicists named above, and likewise of some others. M. Wertheim, in 1844, demonstrated that the electric current and magnetization produce a diminution of the coefficient of elasticity on the bodies which are submitted to their influence; but he thought that he perceived in the sounds produced by these two causes rather a mechanical effect than a molecular phænomenon‡, attributing the sounds which the magnetization determines to the attractive action of the helix on the wire or on the bar of iron, and those caused by the transmitted current to a kind of shock which that transmission effected upon the conducting metal. Admitting, in the case of magnetization, in part the cause pointed out by M. Wertheim, I showed that the molecular action has its own, and that, in the case of the transmitted current, it alone is active. An experiment by M. Guillermin, and the researches of M. Wartmann, confirmed this view, if not on all points, at least on the greatest number. Lastly, in a more recent memoir, I succeeded in showing that, under the influence of a magnet or a helix traversed by a continuous current, all conducting bodies are capable of producing a sound when they transmit a discontinuous current.

In a memoir just published, M. Wertheim, resuming the subject, establishes first, by numerous experiments made with great care, that in magnetization there is a mechanical traction due to a longitudinal and to a transversal component; that the latter becomes null when the iron bar is in the centre

* From the *Annales de Chimie et de Physique* for June 1849.

† Phil. Mag. vol. xxviii. p. 544.

‡ *Comptes Rendus des Séances de l'Académie des Sciences*, vol. xxiii. p. 336.

of the helix, but that in all cases the longitudinal one subsists; and that this force, acting in the direction of the axis, exists equally with a transmitted current. It must produce a longitudinal sound, whether it tends to lengthen or abruptly to shorten the bar; the transversal sound can only arise from the external current, and in an excentric position of the bar. Passing then to the examination of the sounds themselves, M. Wertheim finds their explanation in the mechanical actions which we have just indicated; he does not therefore think that either the magnetization or the transmission of the electric current produces vibrations of a particular kind; but he supposes that the mechanical actions which they engender determine accidentally these longitudinal or transversal vibrations, as any other cause might do. He admits, however, that there is in some cases a dull noise (*bruit sec*), like a kind of crepitation, which is propagated at the moment when the current traverses an iron bar or wire; and he concludes by remarking, that there are still in this class of phænomena many obscure points, especially that which relates to the manner in which a current traversing the iron produces in it a mechanical shock.

M. Wertheim's new observations, of which I have thus given a summary, have induced me to resume my experiments, and to investigate more closely the curious phænomena in question.

It appears to me evident now, that the point which I have sought to establish in my first memoir of 1845*, namely that the magnetization or the passage of the electric current produces a molecular derangement, is no longer disputed. I admit, on the other hand, that I have perhaps attached too much importance to the nature of the sounds produced, and to the influence of certain causes, such as tension, which occasions them to vary. It may be, in fact, that the wire, once set in vibration by either the external or transmitted current, a simple friction against any metallic piece next to the monochord may suffice, if not completely, at least in many cases, to account for the variety of the remarkable sounds which are heard, especially when well-annealed iron wires are used†.

My attention has therefore been especially directed, in

* *Comptes Rendus des Séances de l'Académie des Sciences*, vol. xx. p. 1287; and *Archives de l'Electricité*, vol. v. p. 200.

† The *Comptes Rendus de l'Académie des Sciences*, vol. xxvii. p. 457, contains the extract of a memoir by M. Duhamel on the multiple sounds of bodies, which it appears to me may be well applied to the study of the sounds produced in the iron wires by simple molecular movements. See *Phil. Mag.*, vol. xxxiv. p. 415.

the researches which I have just made, to the investigation of the cause of the fundamental fact, that is to say, of the oscillations which the particles of bodies experience around their position of equilibrium by the action of either external or transmitted currents. With this view, I first submitted to experiment bodies, like iron, susceptible of magnetism, and then other conducting bodies which are not magnetic.

§ I. *Examination of the Vibrations produced in magnetic bodies by either external or transmitted currents.*

On placing very fine iron-filings in the interior of a helix with the axis vertical, these filings are seen to form themselves, under the influence of the current traversing the helix, into small pyramids, elongated in the direction of the axis, which are destroyed and re-formed rapidly when the current is intermittent. The action of the current of the helix on these filings consists, therefore, in distributing them under the form of an elongated thread parallel to the axis,—a thread which the weight alone prevents being as long as the helix itself.

This experiment, which I have already described, and which succeeds as well with very fine iron powder as with iron-filings, proves that the particles of iron under the influence of external magnetization tend to approach each other in the transversal direction, and to extend in the longitudinal direction. It is probable that this phænomenon is due to the form of the elementary particles of the iron, and to the manner in which they are polarized. One thing is certain, that it accounts for what passes in an iron bar or wire submitted to the influence of the intermittent current of the helix. The particles of the iron contending with the cohesion arrange themselves in the longitudinal direction when the current acts, and return to their primitive position as soon as it ceases; from this results a series of oscillations which are isochronous with the interruptions of the current. This manner of viewing the phænomenon entirely agrees with the contraction in the transversal direction accompanied by an extension in the longitudinal direction, which Mr. Joule observed in iron wires and bars submitted to magnetization*.

The same physicist has also remarked that when the wire is much stretched, the magnetization occasions a shortening instead of a lengthening,—an effect which agrees with the cessation of the sounds I have observed when the tension becomes too strong.

All these effects are much more decided in soft iron than

* Phil. Mag. Feb. 1847 and April 1847.

in hardened iron and steel; the cause of which is, that in soft iron the particles are much more mobile around their position of equilibrium. Soft iron and steel magnetized by the action of a magnet or a second helix, exhibit weaker vibrations when the external current tends to magnetize them in the direction in which they are already magnetized, and stronger in the contrary case. The passage of a continuous current through an iron wire modifies the sound which the intermittent action of the external current of the helix causes it to emit, provided that the transmitted current is very strong and the iron very soft.

I shall not enlarge more on this first case, which I think is now well explained; I pass to that of the transmitted current.

An electric current transmitted intermittently through a wire of very soft iron and of small diameter (of 1 to 3 millimetres), determines in it vibrations as strong as those which are produced in it by the same current acting externally under the most favourable conditions. If the iron wire becomes larger, or if it is more hardened, or if it is a steel wire, the effect of the transmitted current is less than that of the external current*. With rods of soft iron, of 4 to 5 millimetres in diameter, the transmission of the continuous current does not completely extinguish, but only diminishes, the effect of the discontinuous current. If the rods are very large, the diminution becomes less perceptible, unless very strong batteries are employed. In the preceding experiments, I made use of two of Grove's batteries of five cells, one for the continuous current, the other for the discontinuous one.

The same experiment made on steel rods and wires gave me precisely contrary results. The sound produced by the transmission of the discontinuous current is weak; but it is increased, instead of being diminished, by the passage of a continuous current passing in the same direction as the discontinuous one. It is a curious fact, that this augmentation remains some instants after the continuous current has ceased to pass; and that it disappears, not all at once, but by degrees and by fits.

I performed these experiments with steel wires of 1 and 2 millimetres in diameter, with rods, both tempered and not tempered, of 3 to 4 millimetres in diameter. The results were the same; the strengthening due to the continuous current is more decided with the rods than with the wires†.

* This experiment of the iron wire covered with silk its whole length, and which gives all the same sounds as when its surface is perfectly naked, would seem to indicate that external causes, such as friction, enter less than is imagined into the production of these sounds: this point deserves to be more closely examined.

† The steel rods are magnetized by the simple passage of these disconti-

It seems to me to result from what precedes, that the effect of the transmitted current is to give to the molecules of the iron a transversal direction, as the effect of the magnetization was to give them a longitudinal one. If the arrangement of the particles of the iron-filings around an iron or any other metallic wire, traversed by an electric current, be examined with attention, these filings are seen to arrange themselves in parallel transversal lines. This is very evident on fixing the conducting wire into a groove made in a wooden board. The filings, being unable to go round the wire, assume a transverse position above it, forming small threads of 3 to 4 millimetres in length, which present opposite poles at their two extremities. When the wire is free, these threads, instead of remaining rectilinear, unite at their two ends, and envelope the surface of the wire, forming around it a closed curve. Now the arrangement which the iron-filings assume around any conducting wire transmitting a current, should be equally assumed by the molecules of the surface of a soft iron wire itself traversed by a current, owing to the effect of this current transmitted through the whole mass of the wire. This is moreover proved by the experiments of Mr. Joule, which show that a soft iron wire or rod experiences a shortening from the effect of a transmitted current. It results, therefore, that when the transmitted current is intermittent, the particles of the surface oscillate between that transversal position and their natural one, and that consequently there is a production of vibrations. These oscillations are the more easy, and consequently the vibrations are the stronger, in proportion to the softness of the iron; with hardened iron, and especially with hard steel, there is a greater resistance to overcome, and the effect is consequently less perceptible. In the first case, the transmission of a continuous current, by impressing on the particles in a permanent manner the position which the discontinuous current tends to give them, must annul or at least materially diminish the oscillatory movement; this in fact takes place. In the case of the hardened iron or steel, the continuous current must, on the contrary, by disturbing the particles from the normal position, without however being able completely to impress on them the transversal direction, on account of the too great resistance which they offer to a displacement, facilitate the oscillatory action of the discontinuous

nuous currents without external current; but they acquire numerous poles. Is this magnetization attributable to the effect of the terrestrial magnetism which the molecular vibrations experienced by the steel would favour, or is it a direct effect of the current transmitted intermittently? New researches would be necessary to solve this question.

current ; experiment confirms this. With regard to the interruptions in the intensity of the sound in this last case, after the continuous current has ceased to pass, they are probably attributable to the fact that the particles, disturbed from their natural position for a longer or shorter time, only return after a more or less prolonged series of oscillations, which the uninterrupted action of the discontinuous current favours.

In aid of the explanation which I have just given, I will add that, having covered a copper wire with an iron envelope which was contiguous to it, and, so to say, plated, I obtained, by passing the discontinuous current through the copper wire, the same effects, excepting intensity, as if the wire had been entirely of iron ; only the sound was not musical, but resembled that which would have been emitted by filings strongly agitated. As this result might be attributed to the fact that a part of the current traversed the iron covering itself, instead of circulating exclusively through the copper wire, I isolated this last by means of a layer of silk or wax, so that the thin cylinder of sheet-iron which surrounded it was not in metallic communication with the copper. The effect was exactly the same as in the preceding case ; that is to say, the copper wire being traversed by a discontinuous current, caused a series of vibrations, or dry and metallic sounds, in the iron covering. This covering underwent, therefore, a transversal magnetization analogous to that which the surface of a wire entirely of iron experiences ; this, indeed, was easily proved from the fact, that the iron-filings were attracted on the two sides of a small longitudinal slit which this covering presented in some parts of its surface, and that the two margins possessed an opposite magnetic polarity. All these effects were more marked when a continuous current, going in the same direction as the discontinuous one, was transmitted through the copper wire ; they ceased entirely as soon as the two currents no longer passed. This increase in intensity of the sound produced by the passage of the continuous current was due to the fact that the discontinuous current had not alone, from acting at a distance, power enough to surmount the coercitive force of the iron covering, which itself was very much hardened, and that it needed the aid of the continuous current to impress a transversal position on the particles of the iron.

Before concluding this first part of my researches, I must again observe, that the passage of a continuous current, passing in a contrary direction to the discontinuous one, diminishes the sound instead of destroying it completely, when the wire submitted to the experiment is of soft iron ; it modifies without

perceptibly weakening it in the steel wires and rods; lastly, it causes it completely to disappear in the iron tube through which the conducting copper wire covered with silk passes. These effects vary, moreover, with the absolute intensity of the currents employed; and they are easily interpreted on the principles which we have stated. We must, moreover, not lose sight of the fact, that when the continuous and discontinuous currents are of equal force, they destroy one another whenever they pass together; so that the discontinuous one no longer acts, and the continuous one becomes intermittent in its action.

Lastly, permanent magnetization materially modifies the sound which the passage of the discontinuous current causes a soft iron wire or rod to emit. In order to guard, in this experiment, against the mechanical effects due to magnetization, I placed a soft iron rod on the two poles of an electro-magnet, taking care by means of an interposed sheet of paper to avoid metallic contact between the poles and the rod. I then placed upon it considerable weights, that its position might not be modified by the magnetization, and passed the discontinuous current through the rod; it gave out a succession of dry metallic sounds, which became much more intense and grave at the moment when I magnetized the electro-magnet. It is evident that this modification and this strengthening of the sound are caused by the contest which is established between the longitudinal position which the influence of the magnetization impresses on the particles of the soft iron, and the transversal position which the passage of the current tends to give them; the oscillations of the particles must necessarily have more amplitude, since they take place between these extreme portions. The effect of permanent magnetization, although still perceptible, is less marked with steel rods, and especially with those of tempered steel.

§ II. *Investigation of the vibratory movements which non-magnetic bodies experience under the influence of external and transmitted electric currents.*

I stated in a preceding memoir, that rods, even of a tolerable size, of different non-magnetic metals emitted a distinct sound when, after having placed them under and very near an electro-magnet or in the interior of the axis of a helix, a discontinuous current was made to pass through them. The sound only became perceptible, whatever was the force of the transmitted current, at the instant when the wire of the electro-magnet or that of the helix was traversed by a continuous current. I also observed that the effect was still more marked when the metal was

in the form of a tube, or of a large wire turned into a helix. I likewise convinced myself, by employing rods of large dimensions, that the production of the sound could not be attributed either to a calorific effect of the current, nor to a mechanical action exerted by the electro-magnet or by the helix upon the conductors traversed by the discontinuous current. I have since made some new observations which sufficiently show that this kind of action is of a particular nature, probably molecular, like that which takes place in magnetic bodies.

Let us first remark, that with a single discontinuous current the phenomenon may be produced without having need in addition of a continuous current or of an electro-magnet. It suffices, for this, to twist the wire which conducts the discontinuous current into the form of a helix. A magnet is thus created; for every time that the current traverses the helix, the latter acquires magnetic properties, and at the same time the wire of the helix is itself a conductor traversed by the discontinuous current upon which the whole of the helix considered as a magnet acts. Moreover, every helix constructed of any metallic wire, the coils of which, whether covered or not with silk, are more or less pressed together, emits a very distinct sound when it is traversed by a discontinuous current. A continuous current, transmitted in the same direction as the discontinuous one, causes the sound to cease entirely, or diminishes its intensity materially, although a notable increase results from it in the electro-magnetic intensity of the helix. This neutralizing effect is probably owing to the fact, that the continuous current impressing permanently on the particles of the wire the position which they should take under the magnetic influence which the helix itself exerts, the discontinuous current no longer causes, at the instant when it is transmitted, new displacements. This effect is perfectly similar to that which is exerted by a continuous current when it passes through an iron wire traversed by a discontinuous current; it causes a cessation of the sound which this wire made under the action of the last current.

But if the continuous current, instead of traversing the wire of the same helix which conducts the discontinuous current, circulates through the wire of another helix which surrounds the first, the effect is totally different. In this case the sound is perceptibly increased, and becomes in general more metallic; this increase is more perceptible in proportion to the weakness of the discontinuous current, and the energy of the ambient continuous current. In this experiment the two currents pass in the same direction; so that the two helices, considered as magnets, present each the same magnetic pole at their same

extremities. If the ambient continuous current proceeds in a contrary direction to the discontinuous one, the sound is weaker than in the preceding case, but stronger than when the exterior helix does not act; it produces likewise a different impression: it resembles the noise of water on the boil, whereas before it seemed to resemble that which a succession of strong sparks would have occasioned. We must remark, however, that if the discontinuous current is powerful enough to cause of itself a sufficiently intense sound to be easily heard at some feet distant, the influence of an external continuous current, when it passes in a contrary direction, diminishes the intensity of the sound. It always increases it, and in all cases, when the two currents pass in the same direction.

The continuous current may also be transmitted through the internal helix, and the discontinuous one through the external helix, contrary to what took place in the preceding experiments. In this case, if the two currents pass in the same direction, the sound resembles a succession of shocks; and when they pass in a contrary direction, the noise is only slightly increased, unless the discontinuous current is produced by a weak battery, for example, by a couple of Grove's cells: then the increase takes place also; only the noise resembles that of water on the boil.

It is easy to understand how a helix traversed by a continuous current, and placed externally or internally to the helix traversed by the discontinuous current, increases the sound which this latter emits. In fact, a permanent magnet is in this case created, the action of which is added to that of the temporary magnet which the discontinuous current creates when it passes through the helix. Moreover, in passing the continuous current through the wire of another helix than that which transmits the discontinuous one, the molecules of the conducting wire do not at first receive the position which they take only by the effect of the passage of the discontinuous current; this it is that causes them to oscillate freely around their natural position of equilibrium. The same result could not be obtained when the two currents passed through the wire of the same helix, as we have already remarked.

When the currents pass in contrary directions in the two helices, it is evident that the result is a diminution in the total magnetism of the whole of the two helices placed in juxtaposition: this is the reason that the sound is not generally increased; and it is only so when the discontinuous current being very weak, the magnetism of the helix which it traverses is so likewise. The magnetism of the other helix, traversed

by the continuous current more energetic than the discontinuous one, then exerts a preponderating influence.

With respect to the difference in the nature of the sound, according as the continuous current passes in the same direction as the discontinuous one, or in a contrary direction, I can only explain it by admitting that the influences of the two magnetisms, opposed and unequal in intensity, diminish the amplitude of the oscillations which the particles of the wire traversed by the discontinuous current make around their natural position of equilibrium,—an amplitude which, on the contrary, is more considerable when the two magnetisms act in the same direction.

I placed a tube of soft iron between the two helices; an increase in the intensity of the sound took place when the two currents proceeded in the same direction,—a diminution and even a complete annihilation when they went in a contrary direction. If the soft iron tube is longer than the internal helix, its presence completely intercepts the influence of the external helix traversed by the continuous current: this is not the case if the tube is slit lengthwise; its presence then in no degree modifies the phænomena. On placing the tube, or a massive cylinder of soft iron in the axis of the internal helix, and no longer between the two helices, the sound is perceptibly increased, especially when the continuous current traverses the internal helix, and the discontinuous one the external helix; the increase takes place even when there is only a discontinuous current. In all cases the nature of the sound does not change; its intensity alone is modified.

A tube of copper or of any other metal produces no effect when placed in the axis of the internal helix. This is not the case if it is between the two helices; it then causes a new sound: this sound is evidently attributable to a current of induction which circulates around the copper tube; for if this tube is slit in its whole length, there is no longer any sound, even when, to increase the magnetic force of the helix, a cylinder of soft iron is placed in its axis.

It is not necessary that the conductors traversed by a discontinuous current should have the form of wires or that of a rod, to give out a sound under the influence of a magnet or a continuous current. A brass disc, 12 centimetres in diameter and 1 millimetre in thickness, was placed horizontally in the centre of a helix, by means of a vertical metallic support which was fixed in the centre of the disc. Care was taken to avoid any contact between the sides of the disc and the internal surface of the helix; the discontinuous current was directed from the centre to the circumference, or from the

circumference to the centre. As soon as a continuous current was transmitted through the wire of the ambient helix, a well-marked succession of metallic sounds was heard in the disc.

Even mercury can produce sounds, as I have already remarked. To render them perceptible, it is only requisite to introduce the mercury into a tube a few millimetres in diameter, and shaped so as nearly to envelope the pole of the electro-magnet. As soon as the latter is magnetized, and the discontinuous current traverses the mercury, a series of sounds is heard similar to those which would result from a regular succession of sparks produced by a strong current between the mercury and a metallic wire. There is not, however, any trace of this, as may be easily ascertained; and, moreover, a current too feeble to produce sparks passed through the mercury is capable of producing the phenomenon. A remarkable fact is, that if, instead of being a little below the polar surface of the electro-magnet, the tube which contains the mercury is upon that surface itself, the sound is not heard. All these effects are very distinct from the movement which the mercury acquires under the influence of magnets, when it is traversed by either discontinuous or continuous currents.

I shall not for the present recur to the remarkable sounds which the voltaic arc occasions under the influence of the electro-magnet,—sounds which I have carefully described in a previous memoir*. They are evidently of the same nature as those of which I have just spoken; for in the voltaic arc the current is, so to say, intermittent from the very nature of the arc which conducts it.

The following, therefore, is the general phenomenon. When any solid conductor, liquid or gaseous (at least very much divided, as in the voltaic arc), is traversed by an electric current,—a magnet, or an assemblage of electric currents closed, and having consequently magnetic properties, acts upon the particles of this conductor so as to give them a relative position different from that which they have naturally. Hence it results that, if the transmitted current is discontinuous, the particles oscillate between their normal position and the forced position which the magnetic influence tends to impart to them; this gives rise to the sounds, and explains the modifications which it presents.

Does the action of the magnetism alone suffice to alter the relative position of the particles of all the bodies, or is the combined action of the magnetism and the electric currents requisite? Faraday's experiments seem to favour the first hypothesis; for the action exerted on light by transparent

* Phil. Mag. vol. xxxi. p. 321.

bodies subjected to the influence of a strong electro-magnet clearly indicate a molecular derangement in them. The recent researches of M. Matteucci, showing that a mechanical action, such as compression, may annul or increase on the same substance (heavy glass, for example) the effect obtained by Faraday by means of an electro-magnet, confirm the fact that this last effect is also a molecular phænomenon.

With regard to non-transparent but conducting bodies, if they are magnetic, they experience a molecular modification under the action of the magnet, as we have stated above. If they are not magnetic, they are diamagnetic; and then it is very probable that the diamagnetism causes a change in the position of their particles, as magnetism does in the others; for it appears to be satisfactorily proved that diamagnetism is attributable to a transversal polarity, as magnetism is due to a longitudinal polarity*. Now whenever any cause produces a polarity in the particles, these must, in obedience to it, arrange themselves in a determined relative position exactly as takes place in the phænomenon of crystallization.

It is therefore probable that, under the influence of a magnet or of an external current, the particles of a diamagnetic body tend to arrange themselves transversely; whilst under that of a transmitted current, they take a longitudinal direction, as is shown by the force of projection which carries the particles of a conductor from the positive to the negative pole, at the point where the circuit is interrupted, in giving rise to the voltaic arc. The struggle between these two contrary tendencies, the one transversal, the other longitudinal, produces the oscillatory movements of the particles around their position of equilibrium, and consequently the vibrations.

In the production of the currents of induction, this molecular derangement which is produced by the magnetic action of a magnet or of a closed current must necessarily take place. To this derangement is probably due the production of an instantaneous electric current; just as when the molecular derangement ceases on the disappearance of the cause which has determined it, there arises a second electric current, having a contrary direction to the first. It is easy to understand why these two currents pass thus in opposite directions.

A conductor placed under the influence of an electro-magnet, or of molecular currents, must therefore be, as long as that influence is exerted upon it, in peculiar molecular conditions. This is shown by the affections of light in those which are transparent; it remains to be demonstrated by other

* The recent researches of Prof. Faraday on the magnetic polarity of crystals of bismuth are quite favourable to this view.

direct means, as to those which are opaque, as Mr. Joule has done for magnetic substances. For the proofs drawn from diamagnetism and sonorous vibrations are only indirect, although the last appear to be tolerably conclusive.

I shall soon return to this subject, in reference to some researches on the relation between diamagnetism and the induced currents, on which I am at present engaged.

LII. *On Systems of Algebra involving more than one Imaginary; and on Equations of the Fifth Degree.* By JAMES COCKLE, Esq., M.A., Barrister-at-Law*.

CONCEIVE two imaginaries, such that their respective squares are equal either to positive or to negative unity. Then the product of two linear functions of these imaginaries is not of the same form as its factors. The product of the imaginaries prevents this similarity, and obstructs the formation of a System of Triple Algebra on the basis just mentioned. But, if we invest the last-named product with the character of a third imaginary, and assume that its square is equal either to positive or to negative unity, four systems of Quadruple Algebra will present themselves, in each of which the product of two linear functions of the three imaginaries will, in general, have the same form as its factors.

Let α and β respectively represent the first and second, and γ the third imaginary. Then $\gamma = \alpha\beta$ or $\beta\alpha$. But as, in quadruple algebra, $\alpha\beta$ is not always equal to $\beta\alpha$, I shall select the former as the expression for γ .

Let A denote a linear function of α , β , and γ ; in other language, let

$$A = w + \alpha x + \beta y + \gamma z,$$

then, in one of the four systems of quadruple algebra above alluded to, the expression A bears the name of a *quaternion*. In the remaining three systems the respective terms *tessarine*, *coquaternion*, and *cotessarine* may be applied to it. At least I have suggested such a nomenclature in No. 1360 of the *Mechanics' Magazine*†, where I have shown the existence of

* Communicated by the Author. In connexion with his paper published at pp. 406–410 of the preceding (34th) volume of this Journal, Mr. Cockle is desirous of referring the reader to two articles subsequently communicated by him to the *Mechanics' Magazine*, and which will be found at pp. 534 and 558, 559 of vol. 50 of that work.

† See pp. 197, 198 of the current (51st) volume of that work. I had, however, previously employed the term “Tessarine” both in that and in the present Journal.

these four systems, discussed them, and pointed out their characteristics. I here propose to advert for a moment to the same subject, to consider it under a slightly different aspect, and also to exhibit, for convenience of comparison, the modular expressions of all the systems. We have, then,

1. The Quaternion System of Sir W. R. Hamilton, in which

$$\alpha^2 = -1, \quad \beta^2 = -1, \quad \text{and} \quad \alpha\beta = \gamma;$$

but in which also

$$\gamma^2 = -1,$$

contrary to what we should have inferred from the equations

$$\gamma^2 = \alpha^2\beta^2 = -1 \times -1 = 1;$$

hence, the quaternion system is abnormal, or does not obey the laws of ordinary algebra. The modulus of a quaternion is the positive square root of the expression

$$w^2 + x^2 + y^2 + z^2.$$

2. The Tessarine System—a normal system in which

$$\alpha^2 = -1, \quad \beta^2 = 1, \quad \text{and} \quad \gamma^2 = -1 = \alpha^2\beta^2.$$

The true modulus of the tessarine A is the positive square root of

$$(w \pm y)^2 + (x \pm z)^2.$$

3. The Coquaternion System, in which

$$\alpha^2 = -1, \quad \beta^2 = 1, \quad \text{and} \quad \gamma^2 = 1;$$

but the last relation is inconsistent with the conditions

$$\gamma^2 = \alpha^2\beta^2 = -1 \times 1 = -1,$$

and the coquaternion system is abnormal. The modulus of A, considered as a coquaternion, is the positive square root of

$$(w \pm y \pm z)^2 + x^2.$$

4. The Cotessarine System, in which

$$\alpha^2 = 1, \quad \beta^2 = 1, \quad \gamma^2 = 1 = \alpha^2\beta^2,$$

and which is a normal system, having for its modular form the positive square root of

$$(w \pm x \pm y \pm z)^2.$$

It is to be borne in mind, that in all the above systems $\gamma = \alpha\beta$; that, whenever the double sign (\pm) occurs, the sign of the term is indifferent and quite independent of that of the preceding or following term; that w , x , y , and z are real quantities, positive, negative, or zero; and that, in multiplying

two expressions of the form A , the modulus of the product is the product of the moduli of the factors. It is for the purpose of analogy and of making the modulus positive, in all cases, that I have given a quadratic form to the formula employed in expressing the cotessarine modulus.

On Equations of the Fifth Degree.

Whether Mr. Jerrard has succeeded in pointing out a method of solving the algebraic equation of the fifth degree or not, his investigations at pp. 545–574 of vol. xxvi. (continued at p. 63 of vol. xxviii.) of the present Series of this Journal must ever be a subject of interest, and form an essential part of the theory of such equations. There are, however, one or two portions of his papers which seem to me involved in doubt and difficulty—difficulty which, in one case, he has himself adverted to and endeavoured to explain. Mr. Jerrard will pardon me if, with great hesitation, I venture to intimate an opinion that the position taken by him in his note [†] to p. 572 of vol. xxvi. is untenable. By way of example, suppose that the square root of $x + h$ is the function to be expanded. The general form of the expansion* is,—

A Series of converging or diverging terms *plus* a Remainder.

Now, when the series is convergent, the remainder may, in all cases where *numerical* value is the subject of inquiry, be entirely neglected; but it does not the less constitute an essential part of the *symbolic* expansion. Hence I conceive that, in considering the expansion under a purely symbolic point of view, even the convergent development must be regarded as incomplete without the remainder, and so placed on the same footing as the divergent one. Considered thus, the convergent and divergent developments are deducible, the one from the other, by an interchange of x and h , and each admits of that interchange without alteration of *symbolic* value. And I think that we *necessarily* obtain an expression which admits of such interchange—at least in all cases where a strictly symbolical expansion is required; and, if I rightly understand Mr. Jerrard's argument, it is to such expansions that his remarks apply.

But, admitting for a moment that the convergent series with the remainder neglected is a *symbolic* expansion of the function,

* I have elsewhere (in the course of my *Horæ Algebraicæ*, *Mechanics'* Magazine, vol. xlvii. p. 150) suggested *contraction* as a term to denote the inverse of expansion. Would it be advisable to confine the terms *expansion* and *contraction* to *symbolic* operation, and to use the terms *involution* and *evolution* exclusively in reference to arithmetic or quasi-arithmetic operations, including them both under the common name *volution*?

I feel some doubt as to another portion of Mr. Jerrard's argument. At all events I think it would be very desirable to show clearly the solvibility of the equation by which W is to be determined. It is true that one of its roots appears to be a known and rational function of another of them, and that an equation among whose roots such a relation exists is supposed to be capable of solution by means of the process of Abel. But doubts—and doubts apparently well-founded—have arisen respecting the universality of that theorem. It is not my object to discuss them here; but I would refer the reader to the learned paper on the Calculus of Functions in the *Encyclopædia Metropolitana*, where, at pp. 327, 328, art. (90.), and at pp. 381, 382, arts. (302.) and (303.) of vol. ii. of the Pure Sciences, he will find remarks upon this question; and I would also call attention to the respective notes to arts. (90.) and (303.) just adverted to. There, the nature of the difficulty which militates against the generality of the theorem—a difficulty which, in the instance of functions of a degree so low as the third, is only obviated by our having complete solution of a cubic—is clearly exhibited.

Standing on the frontiers which separate solvable equations from those as yet unsolved, the biquadratic partakes of the nature of both. It resembles the one inasmuch as it is capable of finite algebraic solution; the other, in its incapability of finite algebraic solution in terms of irreducible biquadratic surds. The latter characteristic might perhaps be of service in the discussion of equations of the fifth degree, and in the manner which I suggested in my First Series of Notes on the Theory of Algebraic Equations, published in vol. xlv. of the *Mechanics' Magazine*. The reader is referred to p. 125 of that volume, and to the condition mentioned at a subsequent page (180) of it.

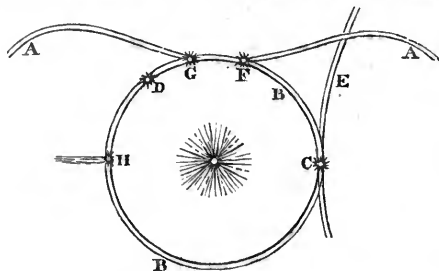
2 Church-Yard Court, Temple,
November 1, 1849.

LIII. *Remarkable Solar Phenomenon seen at the Villa, Beeston near Nottingham, October 22, 1849.* By EDWARD JOSEPH LOWE, Esq., F.R.A.S.*

ON the above day a strange spectacle presented itself about the sun. The morning had been misty, and had cleared up about 22^h; but being engaged with some papers I did not look at the sun until 0^h 10^m, when a remarkable phenomenon was immediately discovered: it resembled a huge

* Communicated by the Author.

pair of wings, AA, 70° in length, very sensibly prismatic, the red being next to the sun, and almost as brilliant as the sun



himself. The sun was pale and sparkling, and the phenomenon brightest directly above that luminary. This was about 25° above the sun.

0^h 13^m. The singular spectacle changed from being *prismatic* to *pure white*; and a circle, BB, of 50° diameter, having the sun for its centre (also colourless), was now visible.

0^h 14^m. A mock sun, C, was apparent although faint, being tinged with prismatic colours, and had a well-defined edge. It was situated on the horizontal level of the true sun on the circle of 50° in diameter, and on the preceding side of the sun.

0^h 15^m. Mock sun C vanished.

0^h 16^m. Another mock sun, D, was faintly visible, formed on the circle above alluded to, and on its left-hand side, at about an altitude of $12\frac{1}{2}^\circ$ above the horizontal level of the true sun.

0^h 18^m. The appearance of wings, together with the mock sun D, disappeared; but an arc of a circle, E, of very large dimensions, became apparent, which cut the former circle at C; it was inverted with respect to the true sun, and must have had its centre on or below the north horizon. It did not remain long enough to take any measurements.

0^h 19^m. Mock sun C again visible, but faint.

0^h 20^m. C vanished; but another mock sun had appeared at F, about 4° below the apex of the circle BB.

0^h 21^m. F, together with BB, disappeared; but the wings AA once more became visible, being again prismatic.

0^h 26^m. BB and C again apparent, and AA more brilliant.

0^h 27^m. A mock sun, H, faintly visible, situated on the horizontal level of the sun, and on the succeeding side of the circle BB.

0^h 27¹/₂^m. H became brilliant and prismatic, having now a tail of 10° in length tapering off to a point, diametrically op-

posite to the true sun; also a mock sun, G, situated 4° below the apex of the circle, BB, on the left-hand side.

0^h 33^m. The winged appearance is the only portion which now remains.

0^h 39^m. AA has just vanished.

During the whole of this phænomenon thin scud was flying rapidly over from the south, and the sky itself appeared of a muddy blue, owing to a thin veil of vapour (probably cirro-stratus) being interposed between us and the clear sky. Whilst this singular appearance lasted, a thin sprinkling of rain constantly fell. The sky became overcast at 0^h 45^m with south wind. Brilliant aurora borealis in the evening.

Villa, Beeston near Nottingham,
October 23, 1849.

LIV. *Inquiries on some modifications in the Colouring of Glass by Metallic Oxides.* By G. BONTEMPS*.

IN the presence of so many illustrious philosophers to whom the sciences are so much indebted, I must certainly apologize for my temerity in daring to call for a few minutes their attention to my humble observations; but if it is true that the greater part of the improvements in manufactures are the consequences of new scientific applications, it will be perhaps admitted that the observation of facts connected with manufactures has led to many new scientific discoveries; and I should feel happy if I could bring before you a few elements of new progress.

The revival of painted windows, and the manufacture of coloured flint-glass, first in Bohemia, and afterwards in all parts of Germany, in France and in England†, in an especial manner directed the attention of glass manufacturers, about fifteen years ago, to the colouring of glass by metallic oxides. They probably tried the receipts described in the works of Neri, Merret, Kunckel, Ferrand, Haudiquier de Blancourt, and many others, and they must frequently have met with failure; in that case their conclusion must have been, that the authors did not obtain the results which they announced. But the truth is, that they had not operated under *like circumstances*. In all cases those receipts had but an empirical value; chemistry was not yet a science; it was merely an agglomeration of facts without any co-ordination whatever; nor was natural philosophy better

* Communicated by the Author, having been read before the British Association at Birmingham, Sept. 1849.

† See Phil. Mag., vol. ix. p. 456.

able to explain the observed phænomena. In more modern times, by the aid of chemical science, we have been able to analyse the metallic oxides, and their various combinations with acids. By analogy, *glass* having been considered as a *salt* with simple or multiple bases, general *axioms* were admitted in the colouring of glass by metallic oxides. It is said, for instance, that the *silicates* of *potash* and of *soda* are *colourless*; the *silicate* of *potash* or *soda* and *manganese* is *purple*; the *silicate* of *potash* or *soda* and *cobalt* is *blue*; the *silicate* of *potash* and *deutoxide* of *copper* is *blue*; the *silicate* of *potash* and *protoxide* of *copper* is *red*; the *silicate* of *potash* and *gold* is *pink*, &c. Such axioms are quite sufficient for those who want only a superficial knowledge; but in entering more deeply into the investigation of the phænomena produced by the use of metallic oxides in glass-making, it will soon be acknowledged how fertile is the field of observations, and how incomplete is their explanation.

Allow me to mention some of the phænomena produced by a few metals; several of them will perhaps have for many persons the charm of new facts, although these metals are those most generally used for colouring glass.

1. *Iron.*

It is generally admitted that oxide of iron gives a greenish colour to glass to the mixture of which it has been added; but the truth is, that this colour is produced *only* in peculiar circumstances.

The manufacturers of china, porcelain and earthenware, are well-aware that oxide of iron is the colouring material of a fine *purplish-red* enamel fired in their muffle (and it is quite clear that *enamels* are real *glass*); if the temperature were raised too high, this enamel would lose its purplish tinge and tend towards *orange*; so that three colours of the spectrum are produced by oxide of iron, even at degrees of heat which I should call *low*, compared with the temperature of furnaces for glass melting, which we shall now consider.

If into a pot containing white melted glass or flint-glass we introduce during the blowing a small fragment of iron, it will, from its gravity, fall to the bottom; now, if after the blowing, this pot is taken out of the furnace, we shall see close to the fragment of iron partly oxidized, a portion of the glass coloured from *orange* to *yellow*. We have also an illustration of the *yellow* colour produced by oxide of iron in the manufacture of artificial *aventurine*. It is known that this *aventurine* is produced by the exposure of soft glass containing a large proportion of the oxides of copper and iron, to a tempera-

ture below its fusion: during this exposure the copper is reduced in the form of metallic crystals, and the glass being coloured only by oxide of iron, takes a *brownish-yellow* colour; and the greater the reduction of copper, the yellower is the glass.

Proceeding now to the usual circumstances of colouring glass by oxide of iron, we find that at a temperature not very high, for instance in covered pots for flint-glass, oxide of iron gives a *green* colour approaching nearer to yellow than to blue. It is generally by mixing oxide of iron with oxide of copper (giving blue) that all the tints of green are produced. The greenish colour of bottle-glass must also be attributed to the oxide of iron combined with the carbonaceous matters contained in the mixture. But when we melt at a high temperature, for instance in the manufacture of window-glass, we remark that the addition of a small proportion of oxide of iron to the mixture produces a glass of a *bluish* colour. It is known also by the manufacturers of bottle-glass, that when the glass is cooled in the pot, it becomes opaque *blue* before being devitrified.

We have shown by the preceding remarks that glass *receives all the colours of the spectrum from oxide of iron*; and at the same time, it will be observed that *these colours are produced in their natural order in proportion as the temperature is increased*.

2. Manganese.

It is generally known that oxide of manganese gives to glass a purple or pink colour, which property is used not only for the production of purple glass, but especially as *glass soap*, for neutralizing the light greenish colour produced by slight portions of iron and carbonaceous matters existing in the materials used in making white glass or flint-glass; but it is very remarkable, that the light pink colour given by oxide of manganese is very apt to fade: if the glass remains too long in the melting-furnace, and afterwards in the annealing kiln, the *purple* tinge turns first to a light *brownish-red*, then to *yellow*, and afterwards to *green*.

I shall mention also a remarkable fact relative to the presence of manganese in the composition of glass. White glass, in which a small proportion of manganese has been used, is liable to become yellow by exposure to light. Having melted for the celebrated Augustin Fresnel the glass for the first polyzonal lenses he made, and for which the whitest glass was desirable, these prismatic pieces of glass became *yellow* after a short time without losing their transparency and polish of

surface. I rightly attributed this colour to the presence of manganese; and, indeed, by suppressing the oxide of manganese in the mixture, this effect no longer took place. Besides, to prove that light had produced this colour, I took a prismatic ring recently made of glass containing manganese: I broke it into two pieces, one of which, exposed to light during a few weeks, became yellow; and the other, kept shut up in a drawer, was not at all altered in its whiteness.

It is also known that some window-panes, especially the Bohemian window-glass, take a light purple colour after having been a long time under the influence of light. The same effect is produced in window-glass or flint-glass containing a small proportion of manganese, when they remain in the flattening or annealing kilns long enough to produce incipient devitrification; in this case the interior of the glass becomes opaque white, whilst the outside takes a pink tint.

I admit that some of the facts of colouring which I have mentioned might be explained by reference to various degrees of oxidation, and that manganese, for instance, loses part of its oxygen when the glass passes from a purple to a yellow colour; but I doubt if this is sufficient to explain the phænomena which I shall call *photogenic*, which take place when the glass is in a solid state.

3. Copper.

Copper in its highest state of oxidation gives to glass quite free from iron a *sky-blue* colour, inclining more to green than to purple, and in its lowest state of oxidation imparts a *ruby* colour. In all times, as at the present day, red window-glass has always been coloured by protoxide of copper; but it is not very easy to obtain this colour, because it is not at all fixed; it must be seized at its proper time; and this production is the origin of a great many interesting and curious observations. When the red glass is in the proper state to be blown, if it is ladled into water so as to effect a sudden cooling, this produces *yellow-green* cullet; if this *yellowish* cullet is heated to the point of liquefaction and cooled slowly, the *red* colour will gradually show itself as the glass cools, becoming of the finest ruby, inclining more to *orange* than to purple: in some cases this colour is so delicate, that the cooling resulting from the usual process of manufacture prevents the manifestation of the red colour, and it is necessary to expose the manufactured piece of glass to the temperature of the annealing kiln, in which case the red colour is seen to increase gradually till it arrives at its greatest intensity: if the temperature of this kiln is too high, or if the ruby glass already

made is placed in a muffle too much fired, the bright *orange-red* colour turns first to *crimson-red*, then to *purple*; by a greater heat it takes a *bluish* tinge, and afterwards gets discoloured; it is therefore acknowledged that ruby glass must be exposed to the lowest temperature possible to obtain the brightest tints. From these observations we conclude that glass in which copper is kept in the state of protoxide by addition of tin or carbonaceous matters, is apt to *acquire successively all the colours of the spectrum*, under circumstances which do not appear to be the effect of modification by oxygen.

4. *Silver.*

Oxide of silver is seldom added to the mixtures which are to be melted in glass furnaces, but is generally used to stain glass of a transparent yellow, on the surface of which it is laid and fired. This colour is produced without any addition of *flux*; it is only necessary to lay on the surface of the glass or flint-glass a small proportion of oxide, or any salt of silver in a great state of division, mixed with a neutral medium, such as pounded clay or red oxide of iron, and to expose this glass to the heat of a muffle; the medium is afterwards taken off by brushing the surface of the glass, and the glass is stained of a yellow colour, which varies between *lemon* or *greenish-yellow* and dark *orange*, according to the quantity of silver, and especially to the quality of the glass; a *red* colour can even be produced by exposing the glass twice to the heat of the muffle. The celebrated Dumas has found by accurate analysis, that the glass which was liable to take the deep tints had its elements the nearest in definite proportions; which agrees with this observation, that the glass must have been deprived of all excess of alkali by a long melting at a high temperature, to take the deep tints of *orange* and *red*.

It is important not to heat the muffle to too high a temperature, otherwise the surface of the glass on which the silver has been laid becomes opalescent, although when seen through it still remains yellow or orange: the glass viewed obliquely reflects an opaque blue colour, and at a still higher temperature it is liable to appear of a *pink* colour when seen through, although the opacity of the surface is still increased, and becomes brownish-yellow.

If, instead of staining the glass in a muffle, silver added to a mixture of flint-glass is melted in covered pots in the shortest time possible, the result is an agatized semi-opaque matter, which, by the combined effects of refraction and reflexion, *will present all the colours of the spectrum*; this effect is most sensible, if the surface of the glass, which is generally yellowish-

green opake, is cut to different depths. These effects are produced by inequalities of cooling, as we have seen for manganese and copper.

5. Gold.

Oxide of gold gives to the glass a pink tint, which by an increase of quantity may attain a *purplish-red*. For this purpose a small proportion of precipitated purple of Cassius is added to the mixture of flint-glass; but by the first melting this mixture gives only a colourless transparent glass, which must be heated again to show the pink colour. If, for instance, a small solid cylinder has been formed with this first melted glass, when cold it is quite white; but if this cylinder is afterwards exposed to the heat of the working-hole of the furnace, we see it acquire the red colour gradually as it is penetrated by heat; and this colour remains fixed when the cylinder is gradually cooled again in the annealing kiln.

I have remarked also, that by varying the degrees of heating a piece of this glass of some length at a high temperature, and re-cooling it several times, a great number of tints, varying from blue to pink, red, opake yellow and green, may be produced. But I am not certain that this effect might not be attributed to some fractions of silver mixed with the gold used; and the only point that remains quite positive, is the fact of the pink colour *showing itself by a second firing* in the glass into the composition of which gold enters.

To these results of colouring by metallic oxides, I shall add an effect produced in the colouring of glass by charcoal, which effect is of the same nature as those mentioned in the colouring by copper and gold.

An excess of charcoal in the mixture of a silico-alkaline glass gives a yellow colour, which is not so bright as the yellow from silver, but good enough to be used in church windows; and sometimes, according to the nature of the wood from which the charcoal has been made and the time at which it has been cut, this yellow colour *may be turned to dark red by a second fire*.

I doubt, indeed, whether all the results which I have mentioned can be explained only by various degrees of oxidation of the metals. This multiplicity of colours, greater than the number of oxides described for each metal, must lead us to consider whether those phænomena are not the consequence of *physical laws*. It is the peculiar character of our time, and the result of the immense progress accomplished in chemistry and natural philosophy, to bring their study to some united views, which render the connexion of these two sciences indissoluble.

The various facts observed in the colouring of glass, which are especially produced by the influence of different temperatures, are probably to be *attributed to some modifications in the disposition of the composing particles*; which effects occasion modifications in the reflexion and refraction of the rays of light: indeed it might be remarked, that parts of the results which I have mentioned are *produced under some circumstances which appear to place the glass in a condition of crystallization*.

In the last century, Edward Hussy Delaval, starting from the experiments made by the immortal Newton in the colouring of thin plates, instituted some researches into the causes of the modifications of colours in bodies; but he found chemical science not in a state sufficiently advanced to establish his observations on rational experiments. But at the present time we have only to collect a sufficient number of precise facts to be able to deduce from them the scientific explanations, which might probably lead to some new improvements in manufactures.

As for glass, the observations relating to the constitution of its particles are extremely delicate. This is proved by the difference of the action of light on it, according to the degree of annealing. It is known that even a very slight pressure, acting on a point of its surface, is sufficient to produce the doubly refracting power, which is also given by incomplete annealing; and this effect takes place, not only when the glass, having been quickly cooled from a red heat to the ordinary temperature, is liable to break by itself, but even in pieces of glass of some thickness, which might be considered to be well annealed, and which would really be sufficiently annealed for common use: it is a fact, that the greatest part of such a glass shows sensibly the phenomena of polarization. This fact has still increased the difficulties, which were already very great, in manufacturing glass for optical purposes. The difficulty, which is not a small one for discs of three or four inches in diameter, is of course greater for discs of ten and twelve inches; we have however surmounted it at Messrs. Chance's glass works for discs up to twenty-four inches: but before working such discs, or larger ones, we think that it would be desirable that practical opticians should throw sufficient light on the various parts of the processes which are used in the construction of achromatic telescopes; because we could not warrant that the glass which we consider to be free from defects, may not, with very high magnifying powers, give evidence of new imperfections which we have not yet suspected.

I have laid before you practical facts. If they be found interesting enough to form the basis of new studies on the

modification of the atomic constitution of glass, I shall be content to have brought the subject before the British Association: if these observations, on the contrary, are considered not worthy of the importance I attach to them, I shall have my excuse in the love of an art to which I have all my life been zealously devoted.

LV. *On the Cause of Auroræ Boreales.* By AUGUSTE DE LA RIVE, being an Extract from a Letter to M. Regnault*.

I HAVE just read, in a memoir by M. Morlet on the *Auroræ Boreales*, inserted in the *Annales de Chimie et de Physique*, 3rd series, vol. xxvii. the following passage:—

“With regard to the origin of this luminous matter (that of the *aurora borealis*), it seems natural to attribute it to the electric fluid contained in the atmosphere, and which, at great heights where the air is rarefied, must become luminous as under the receiver of the air-pump and in the barometric vacuum: this hypothesis would acquire a great probability if we succeeded in proving, by direct experiments, that magnetism exerts an influence on electric light.”

This last expression induces me to request you to have the goodness to communicate to the Academy of Sciences an experiment which I mentioned to you on my passage through Paris last June, and which you may perhaps remember; its object was to show, in support of the theory which I had advanced of the *aurora borealis*, the influence exerted by magnetism upon the light which is produced in ordinary electric discharges. Hitherto this influence has only been shown in the case of the luminous arc which escapes between two conducting points, each communicating with one of the poles of a voltaic battery; which is very different, both as concerns the phænomenon itself, and in what concerns its application to the theory of the *aurora borealis*. The following is my experiment.

I introduce into a glass globe about 30 centimetres in diameter, by one of the two tubulures with which it is furnished, a cylindrical iron bar, of such length that one of its extremities reaches nearly to the centre of the globe, whilst the other extends from 3 to 4 centimetres out of the tubulure. The bar is hermetically sealed in the tubulure, and covered throughout its length, except at its two ends, with an isolating and thick layer of wax. A copper ring surrounds the bar above the isolating layer in its internal part the nearest to the side

* From the *Comptes Rendus* for Oct. 15, 1849.

of the globe; from this ring proceeds a conducting rod, which, carefully isolated, traverses the same tubulure as the iron bar, but without communicating with it, and terminates externally in a knob or hook. When by means of a stop-cock adjusted to the second tubulure of the globe, the air in it is rarefied up to 3 to 5 millimetres, the hook is made to communicate with one of the conductors of an electric machine, and the external extremity of the iron bar with the other, so that the two electricities unite in the interior of the globe, forming between the internal extremity of the iron bar and the copper ring which is at its base, a more or less regular fascicle of light. But if the external extremity of the iron bar is placed in contact with one of the poles of a strong electro-magnet, taking good care to preserve the isolation, the electric light takes a very different aspect. Instead of issuing, as before, from the different points of the surface of the terminal part of the iron bar, it is emitted only from the points which form the contour of this part, so as to constitute a continuous luminous ring. This is not all: this ring, and the luminous jets which emanate from it, have a continuous movement of rotation around the magnetized bar; one while in one direction, at other times in another, according to the electric discharges and the direction of the magnetization. Lastly, more brilliant jets appear to issue from this luminous circumference without being confounded with those which terminate on the ring, and form the fascicle. As soon as the magnetization ceases, the luminous phænomenon becomes again what it was previously, and what it is generally in the experiment known by the name of the *electrical egg*. Not having any powerful machine at my disposal, I used for my experiment an Armstrong's hydro-electric machine, the boiler of which I made to communicate with the copper ring, and the isolated conductor which receives the vapour with the iron bar, or *vice versâ* when I wished to change the direction of the discharges. The experiment succeeded very well in this manner.

The experiment which I have just described appears to me to account very satisfactorily for what passes in the phænomenon of the aurora borealis: in fact, the light which results from the union of the two electricities in the part of the atmosphere which covers the polar regions, instead of remaining vaguely distributed, is carried by the action of the terrestrial magnetism round the magnetic pole of the globe, whence it seems to rise in a revolving column, of which it is the base. We thus understand why the magnetic pole is always the apparent centre whence issues the light constituting the aurora borealis, or toward which it appears to converge. I shall not

recur to the other circumstances which accompany this meteorological phenomenon, the agreement of which I have shown with the explanation I have given in a letter addressed to M. Arago, which was communicated to the Academy, and inserted in the *Philosophical Magazine* for April 1849, p. 286.

But, having referred to this letter, in which the question was also raised respecting the explanation of the diurnal variations of the magnetic needle, permit me to add, that I have had occasion to prove, in England, both by my own observations, and still better by the more extensive ones of several physicists*, the existence of electric currents having a direction from the north-west to the south-east on the surface of the earth. The presence of these currents can be easily proved by means of the metallic wires which serve as telegraphic communications: wires which are placed underground and at the same time well-isolated, except at their two extremities which dip into the ground, are best suited for this kind of observations. It is very curious to follow the agreement which exists between the variations of intensity of these currents and the variations of magnitude of the deviation of the magnetic needle of declination; a new proof to add to that drawn from their direction, that they are the cause of the diurnal variations.

Colonel Sabine has stated, in opposition to my explanation of the diurnal variations, an objection drawn from the observation of these variations at the Island of St. Helena and at the Cape of Good Hope†. I do not think it well-founded, and, without entering into the details which will better find a place elsewhere, I shall limit myself to one single remark. I attribute the origin of the currents which give rise to the aurora borealis and to the diurnal variations, to the rupture of the electric equilibrium occasioned, in each atmospheric column, by the difference of temperature which exists between its base which reposes on the surface of the globe and its upper part which is at the limit of the atmosphere. Each column thus forms a kind of pile charged at its two extremities with contrary electricities, which unite in part by the pile itself, in part by a circuit formed of the upper regions of the atmosphere, of the atmospheric polar regions, and of the surface of the earth. Meteorological circumstances determine the greater or less proportion of the two electricities which unite by one or other of these ways.

Now, the temperature of the base of the column must vary

* See the observations of Mr. W. H. Barlow on this subject, *Phil. Mag.* vol. xxxiv. p. 344.

† *Phil. Mag.* vol. xxxiv. p. 466.

not only with the season, with the time of the day, and with the latitude of the place where it is observed, but also with the nature of the surface of the globe on which it reposes. When, therefore, this surface is the sea, the hours of maxima and minima of temperature are not the same as when it is *terra firma*, all other circumstances being the same; it results necessarily that the hours of maxima and minima of intensity of the electric currents, and consequently of the diurnal variations to which they give rise, must be equally different. Now, St. Helena and the Cape of Good Hope may be considered as places enveloped in atmospheric columns, which have almost their entire base resting on the sea and not on the land; thence the anomalies pointed out by Colonel Sabine are very easily explained, and, in particular, it is easily understood how there is no agreement, in direction, which must in every case be different, between the diurnal variations observed at the Cape of Good Hope and those observed at Algiers, which is equally distant from the equator, but to the north. An excellent paper by M. Aimé on terrestrial magnetism, inserted in the *Annales de Chimie et de Physique*, 3rd series, vol. xvii., in which he discusses comparatively the observations made at St. Helena, the Cape, and Algiers, has singularly facilitated the explanation of the anomalies presented as objections by Mr. Sabine.

I, however, do not pretend that there does not exist any anomaly; my explanation is not more free than others from those which result from certain local and exceptional causes. I am not further from admitting that the currents of induction determined on the surface itself of the globe, by its rotation under the influence of its magnetic poles, cannot have any part in the phænomenon of the diurnal variations and auroræ boreales, and account for the connexion which these variations appear to have with the absolute direction both in declination and in inclination of the magnetic needle, and with the absolute intensity of the terrestrial magnetism. But this subject would require, for elucidation, to be treated more at length than can be done in a letter; I shall therefore stop, and beg to refer those persons who may be interested in this question to a memoir which I am on the point of completing, and which will be published forthwith.

LVI. *Descriptions and Analyses of several American Minerals.* By B. SILLIMAN, Jun., M.D., Professor of Chemistry applied to the Arts in Yale College, and of Medical Chemistry and Toxicology in Louisville University, Kentucky*.

THE results embodied in this article have been lately obtained in the Analytical Laboratory of Yale College by myself, or by my pupils under my immediate supervision and direction.

The researches upon the new and interesting species which belong to the family of micas is not complete; but as many months must pass before I can again take up this investigation, it is deemed best to present the results already obtained, that the attention of mineralogists may be directed to them. I will present in a second memoir such further results as may be determined by the analyses which will be carried forward this winter on the same species. Enough has been done, it is believed, to give definiteness and importance to the subject.

I. SPECIES OF THE FAMILY MICA.

This series of minerals, forming a new and very interesting addition to the mica family, is found associated with the corundum of Pennsylvania; and one or more of the species are probably associated with corundum in every locality where the latter is found. My attention was first called to these minerals by receiving from Dr. J. L. Smith, now in Constantinople, a small portion of a similar mineral, which he has called *Emerylite*. The quantity of this mineral received (only 0.2 grm.) was too small to enable me to obtain more than its general characters. As this mica was the means of calling my attention to the others, I will repeat the results of Dr. Smith, with such additional characters as were obtained here.

Emerylite.

This mineral is found associated with the emery from the localities of Asia Minor. It is in brilliant micaceous scales, brittle and inelastic. Colour, gray with a tinge of lilac; laminæ easily separable; hardness, 3 to 3.25; gravity not satisfactorily determined on so small a quantity. Before the blowpipe alone in forceps, exfoliates, whitens and emits a very brilliant light, but does not fuse. In close tube, yields water, which gave a feeble reaction for fluorine. Dissolves in borax to a clear glass, and leaves a siliceous skeleton in salt of phos-

* From Silliman's Journal for November 1849.

phorus. The reactions for silica, alumina, lime, iron and potash are satisfactory. It is not acted on by strong acids; even by long-continued boiling with Nordhausen sulphuric acid, very imperfect decomposition was effected.

Fused with carbonate of baryta, a qualitative analysis gave reactions for silica, alumina, peroxide of iron, lime and potash, with a trace of soda.

I was unable, however, with the most exact care, to confirm Dr. Smith's observation of the existence of zirconia—probably a larger portion of the mineral might give a different result.

Dr. Smith gives the following as an approximate result of the constitution of the emerylite from several analyses made by himself:—

Silica	30.0
Alumina	50.0
Zirconia	4.0
Lime	13.0
Oxide of iron, manganese and potash	3.0
		<hr/> 100.0

This analysis gives the ratio $4\text{SiO}^3, 6\text{AlO}^3, 3\text{RO}=3\text{RO}, \text{SiO}^3+3(\text{AlO}^3, \text{ZrO}^3)^2 \text{SiO}^3$, which gives the following result:—

4 atoms Silica	. .	2309.24	=	31.93	per cent.
6 ... Alumina	. .	3854.00		53.30	
3 ... Lime	. .	1068.06		14.77	
		<hr/> 7231.30		100.00	

As however the mineral contains water and the analysis is confessedly only approximate, this formula cannot be regarded as entirely correct; but it will be found useful in connexion with the results which follow.

The mineral which most closely approaches Smith's emerylite, as far as our observations authorize us to form an opinion, is the next in order, and marked in our analyses "A."

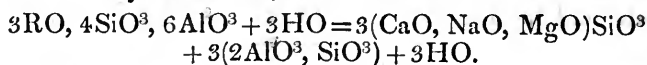
A. This mineral is from Village Green, in the town of Aston, Chester County, Pennsylvania, and was sent to me by Mr. L. White Williams of West Chester, to whom mineralogists are much indebted for bringing to light many interesting things. It is associated with corundum, and occurs in considerable masses; and so much resembling common mica, as to have escaped notice until Dr. Smith's observations on emerylite called my attention to the minerals associated with the American corundums. Form, like mica, apparently hexa-

gonal; foliæ easily separable, but inelastic and brittle. Colour white; transparent in thin foliæ. Lustre, silvery, vitreous and pearly. Hardness, 3·5. Gravity, 2·995. B. B. in forceps, exfoliates and emits a strong light; fuses on the edges of thin laminae. In the close tube it yields water, and gives very feeble traces of fluorine. It behaved with the fluxes like the Turkish mineral. A qualitative analysis showed the presence of silica, alumina, lime, magnesia, soda, a trace of potash and iron, water and fluorine, the last in very feeble quantity.

The quantitative analysis of this species is still incomplete as to its alkaline constituents, which are given by the difference, and the amount of water is probably placed too high*. The analysis was conducted, under my direction, by my pupil, Mr. W. J. Crawe. Three analyses gave him as follows:—

	I.	II.	III.	Oxygen.
Silica	32·311	31·060	31·261	16·24 = 4
Alumina . . .	49·243	51·199	51·603	23·74 6
Lime	10·663	9·239	10·146	} 3·42 1
Magnesia . . .	0·298	0·283	0·499	
Soda and potash†	2·215	2·969	1·221	
Water	5·270	5·270	5·270	4·72 1
	100·000	100·000	100·000	

It is obvious that this is very nearly the true constitution of the mineral. The following formula corresponds very closely with the analytical results, viz.—



4 atoms Silica . .	2309·24	30·51 per cent.
6 ... Alumina . .	3854·00	50·92
3 ... Lime . .	1068·06	14·11
3 ... Water . .	337·44	4·46
	7568·74	100·00

This result leaves but little doubt that the mineral here examined is the same as the Turkish emerylite. That the American species will be found constant in containing water I have no doubt.

Great care was bestowed on the trials to detect zirconia, but none was found.

Corundellite.

The next mineral belonging to this series I have called Corundellite. This species in external characters much re-

* The mean of two determinations.

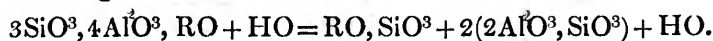
† By the difference.

resembles the last, but its composition is different in important particulars. It is also found associated with the corundum and emery of Unionville, Chester County, Pennsylvania. The specimen here analysed is marked "D," and was taken by me in May last from the mineral collection of the Chester County Cabinet formed by Mr. Williams. It is in broad foliated masses of a yellowish-white colour, easily cleavable, and apparently hexagonal in form, penetrated by hexagonal crystals of corundum. Inelastic, brittle; resembles common mica, but not so strikingly as A. Hardness, 3.5. Gravity, 3. B. B. gives the same characters as the last species. No reaction could be obtained for lithia or boracic acid in any of the minerals of this series. The reaction for fluorine in this one was feeble. It is unaffected by strong acids even on long boiling, except partially by very strong sulphuric acid. Its qualitative analysis gave silica, alumina, lime, potash, soda and water, with a trace of iron and fluorine.

The following analysis was made by Mr. J. J. Crooke, on 1.389 grm. of the substance fused with carbonate of baryta. It yielded—

					Oxygen.		
Silica	0.496	= 35.708	p. c.	18.553	= 18.55	= 9
Alumina	0.738	53.131		24.872	24.87	12
Lime	0.101	7.271		2.042	} 2.36	1
Potash	0.017	1.224		0.207		
Soda	0.006	0.413		0.110		
Water and fluorine		0.032	2.303		2.050	2.05	1
					<hr/>		
					1.390	100.068	

This gives the ratios



	Atoms.	Required.	Found.
3 atoms Silica . .	1731.94	= 36.31 per cent.	35.708
4 atoms Alumina . .	2569.32	53.87	53.131
1 atom Lime . . .	356.02	7.46	8.926
1 atom Water . . .	112.48	2.36	2.303
<hr/>			<hr/>
4769.76			100.068

This species somewhat resembles margarite, and it may be shown on further examination that margarite is a hydrated mineral. At present it is reported as anhydrous, and its proportions of silica and alumina are different from the present species. Its analysis, given by Hausmann on the authority of the Göttingen Laboratory, is—

Silica . . .	33.50 = 8 atoms.	Silica . . .	4618.48	34.47 p.c.
Alumina . . .	58.00 12	Alumina . . .	7708.00	57.55
Lime . . .	7.50 3	Lime . . .	1068.06	7.98
Protox. iron . . .	0.42			
Manganese . . .	0.03			
Magnesia . . .	0.05			
	<hr/>			
	99.50		13394.54	100.00



Possibly a new analysis may bring these species together.

The species corundellite occurs not only in the broad foliated masses above alluded to, but also in small scales disseminated throughout the mass of granular corundum at Unionville, Pennsylvania, and in this form is quite abundant. Not unfrequently these scales have a delicate shade of violet, especially when wet. The rock is difficult to break, and the corundellite appears to adhere very strongly to the associated minerals, and the laminæ are not so easily separable as in the foliated masses*.

Euphyllite.

This beautiful pearly white mineral is found associated with black tourmaline and corundum at Unionville, Pennsylvania. Form, apparently hexagonal; cleavage, eminent on basal plane; the laminæ not so easily separable as in mica. Hardness, 3. Gravity, 2.963. Lustre of sides, faint pearly; of basal plane, very brilliant pearly, resembling Heulandite, but perhaps more brilliant even than in that species. Colour of cleavage, face pure white, of sides grayish, sea-green or whitish. Laminæ, rather brittle, inelastic, and quite transparent.

B. B. exfoliates, fuses on edges of thin laminæ, and emits a stronger light than either of the corresponding species. In the matrass it evolves water, and gives a reaction for fluorine. No reaction for lithia or boracic acid was obtained, but it gives a soda-yellow to the flame.

The qualitative analysis of this mineral gave silica, alumina, lime, magnesia, soda, water and fluorine.

The quantitative analysis was conducted by Mr. J. J. Crooke,

* The species barsowite (G. Rose) appears in the Ural to hold the same geognostic relations to corundum as do the minerals of the present memoir in this country. Its composition however is quite distinct (silica, 49.01; alumina, 33.85; lime, 5.46; magnesia, 1.55 = 99.87, Varrentrapp), while its hardness, 6, and absence of micaceous structure, render it entirely distinct. It approaches scapolite in composition, but with a smaller quantity of protoxide. I am led to allude to this species from the fact, that an intelligent foreign mineralogist, to whom I showed some of the corundellite, remarked that there appeared to be a similarity between the species. There is however a most marked difference, in that corundellite is a mica.

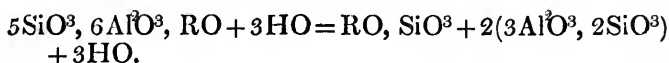
and gave on fusion with carbonate of baryta the following results, viz. quantity taken, 1·378 grm.; found—

			Oxygen.	
Silica . . .	0·538 = 39·042 per cent.		20·28 = 15	
Alumina . . .	0·708 51·378		23·99	18
Lime . . .	0·044 3·193	0·897	} 1·54	1
Magnesia . . .	0·015 1·088	0·421		
Soda . . .	0·012 0·871	0·223		
Water . . .	0·063 4·593		4·08	3
	1·380 100·165			

This gives the following as the theoretical composition of the mineral :—

5 atoms Silica	2886·55 = 39·02 per cent.	
6 atoms Alumina	3854·00	52·10
1 atom CaO, MgO	319·38	4·32
3 atoms Water	337·44	4·56
	73·97·37	100·00

The following formulæ therefore express its constitution :—



The alumina obtained in this analysis (as well as in all the others) was very critically examined for zirconia, but without success.

The black tourmaline which is associated with euphyllite has left the impression of its crystals on the lateral face of the mineral with such a smooth hard-looking surface that it shows no trace of a micaceous structure. The tourmaline has an uncommon form, the faces R of the primary form being rudimentary from the extension of the tangential plane, truncating the summit.

The beautiful foliæ of this pearly white mineral have suggested the name euphyllite as an appropriate designation for the species, while the name corundellite has the same obvious derivation as emerylite, the mineral described by Dr. Smith.

There is a similar mineral associated with the blue corundum of North Carolina, which was made known to mineralogists by the Hon. T. L. Klingman, M.C., from North Carolina. It occurs investing the corundum. Colour, faint olive-brown. Lustre, vitreous to pearly, like mica. In cleavable plates, apparently hexagonal. Cleavage, perfect; laminæ, separable. Hardness, 3. Gravity, 2·94 to 3·008. Brittle, transparent, not acted on by strong acids. B. B. whitens, gives a brilliant

light, but does not fuse, unless with great difficulty, on the edges. It contains a trace of fluorine, and a qualitative analysis detected in it silica, alumina, lime, soda and water. An insufficient quantity of the mineral prevented a perfect analysis being made. So far as its constituents have been obtained, it contains,—silica, 36.369; alumina, 42.373; lime, 10.141; magnesia, 4.462; water, 1.448; the difference, soda and loss. Soda, about 4 per cent.

Should it appear, on repeating the analysis of this mineral, that it is new, as the present would appear to indicate, I would propose to adopt the name *Clingmanite*, suggested by Prof. Shepard, in honour of the distinguished gentleman before named, who has shown great interest in advancing the study of mineralogy*.

I have had no means of comparing the optical properties of these several minerals. The angle between their axes of polarization should be measured to ascertain if the differences shown in their composition are found also in their molecular structure. When we review the characters of the minerals here described, we are struck with the almost identity of all their ordinary physical characters; and yet there are differences which are apparent, especially in their composition. It therefore becomes an interesting question, to decide if the optical characters will sustain the chemical results. The occurrence of a class of salts with such a very small amount of protoxide bases, and so large a quantity of alumina as these possess, is a novelty in the chemical history of minerals, and may have some important theoretical connexions. Our knowledge of the whole mica family is quite imperfect at present. The true function of the fluorine found in so many of them yet remains to be explained; and especially is it of the greatest importance that a careful series of optical measurements should be made on authentic specimens from numerous localities, and at the same time an exact series of chemical analyses conducted on specimens from the same localities.

Mineralogy hardly offers a more inviting investigation than this; and should it not fall into better hands, it will at a future day be attempted in this laboratory.

* Prof. C. U. Shepard had noticed this mineral, and supposing it to be new, he had determined to give it the above name. When he found, however, that I was engaged on this series of minerals, he promptly abandoned the investigation. At that time we both thought that the emerylite of Dr. Smith would probably include all the American species herein described, which now appears not to be the fact. On going to England in June, Prof. Shepard left me a memorandum containing his notes on the North Carolina mineral, and I have embodied them in the above description with my own.

II. ON UNIONITE.

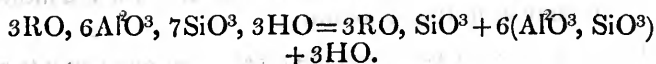
The next mineral to be noticed is from the same specimen which furnished me the euphyllite. In general appearance it somewhat resembles scapolite or spodumene. It is implanted in black tourmaline, and is intimately associated with the euphyllite. Its form is discernible only by its cleavages, which are distinct in one direction, the planes dividing the mineral into parallel laminæ; in two other directions less distinct, but yielding a form probably triclinæ*. Lustre, vitreous. Colour, yellowish-white to white. Hardness, 6 to 6.5. Gravity, 3.2984. Brittle, and easily reduced to powder. In acids does not gelatinize.

B. B. in forceps, it whitens, swells up and fuses to a white enamel, giving out at the same time an extremely brilliant light. In the matrass it gives out water which is acid, and the glass is etched with hydrofluoric acid. Qualitative analysis detected silica, alumina, magnesia and soda. The amount of water was determined by the loss on heating, and the fluorine was not separately estimated. In the quantitative analysis the mineral was attacked by carbonate of baryta.

The following are the results of analysis. Taken, 0.7335 grm. Yielded—

				Oxygen.
Silica	0.32385	=44.151 p.c.	22.940=7
Alumina	0.31000	42.263	19.763 6
Magnesia	0.05400	7.361	2.85 } 0.46 }
Soda	0.01270	1.731	
Water and fluorine		0.02590	3.532	3.290 1
Loss	0.00705	0.962	3.146 1
		0.73350	100.000	

7 atoms Silica.	4041.17	= 44.86 per cent.
6 atoms Alumina.	3854.00	42.78
3 atoms Magnesia	775.06	8.62
3 atoms Water	337.44	3.74
		9007.67	100.00



This formula and constitution are believed to be unknown in any previously noticed species among minerals, and I therefore propose it as new, and suggest for it the name Unionite, derived from Unionville, its locality. At present it is a rare substance, but I understand that the place where it was found

* The angles do not admit of measurement.

is to be worked soon for emery, and probably both it and the euphyllite will be obtained there in abundance. This species was also supplied to me by Mr. Williams of West Chester.

III. ON MONROLITE, A MINERAL RESEMBLING WÖRTHITE.

My attention was called to this mineral by Mr. Wm. S. Vaux of Philadelphia, who had received it from the locality marked "topaz." It somewhat resembles pycnite in general aspect, but as will be seen is a very different thing.

It occurs at Monroe, Orange Co., New York, where it is found in a quartzose rock with magnetic iron, pink felspar, black mica, pinite and common garnet. Colour green to greenish gray. Structure radiating in sheafs from a centre in groups from an inch to two inches in diameter. Also in single implanted individuals. Cleavage and form of single crystals resembles Sillimanite. Hardness, 7·25 on an angle; on cleavage face about 6. Gravity, 3·045, 3·096, 3·07. Columnar, fibrous. The oblique prisms were not measured, being too irregular.

B.B. alone in tube gives off neutral water. Infusible, whitens; dissolves slowly in carbonate of soda, readily in borax and in salt of phosphorus, leaves a siliceous skeleton which reacts slightly for iron.

Its qualitative assay indicated the presence of silica and alumina, with a trace of iron and magnesia.

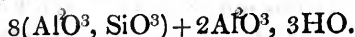
It was fused with carbonate of potash and caustic potash, and its analysis yielded—

	I.	II.	III.
Silica . . .	40·92	40·389	40·389
Alumina . . .	56·61	55·729	56·618
Magnesia . . .	·28	·280	·280
Water . . .	3·09	1·840	2·794
	<hr/> 100·90	<hr/> 98·238	<hr/> 100·079

These analyses correspond closely with 8SiO^3 , 10AlO^3 , 3HO .

8 atoms Silica . .	4618·48	=	40·59	per cent.
10 atoms Alumina . .	6423·30		56·44	...
3 atoms Water . .	337·44		2·97	...
	<hr/> 11379·22		<hr/> 100·00	

We have then the formula



The Worthite of Hess gave the formula—

$5(\text{AlO}^3, \text{SiO}^3) + \text{AlO}^3, 3\text{HO}$ corresponding to his analysis, viz.

Silica	40.79
Alumina	53.06
Water	4.63
Magnesia	0.88
	<hr/>
	99.36

I have never seen the Wörthite, and have therefore no means of judging of the similarity of these two minerals in other respects. The probability of being able to refer the present mineral to kyanite seemed to me at first quite strong, but I was unable by any care to procure an amount of silica less than that given in these analyses. Should this mineral on further examination and comparison prove to be distinct, I propose for it the name Monrolite, derived from the locality where it was found.

IV. ON THE IDENTITY OF SILLIMANITE, FIBROLITE AND BUCHOLZITE WITH KYANITE.

Sillimanite was originally described by Bowen*, from an analysis made in Yale College Laboratory in 1825, which showed it to be a silicate of alumina with a proportion of silica too high to allow it to come within the formula of kyanite. It was subsequently analysed by Dr. Thomas Muir, in the laboratory of Dr. Thomson, who found in it a large quantity of zirconia, an observation which all subsequent researches have failed to confirm. Since that time it has been analysed by various chemists; viz. by Connel, Norton, Staff, Hayes and Thomson. The most recent of these analyses which has been published is that by Thomson, who reports it to contain 45.65 per cent. of silica. We have then the following discordant results in the amount of silica found in Sillimanite by different chemists in the order of their publication:—

	1. Bowen.	2. Muir.	3. Connel.	4. Norton.	5. Staff.	6. Hayes.	7. Thomson.
Per cent.	42.67	38.67	36.75	37.40	37.36	42.60	46.65

The cause of this disagreement will undoubtedly be found in the difficulty of effecting a complete decomposition of anhydrous silicates of alumina, which contain a high per-centage of alumina. This decomposition can be completely effected only by the aid of caustic potash applied to the mixture of carbonates and the mineral during the fusion, as first recommended by Berzelius, or by hydrofluoric acid.

Select crystals of this mineral were taken from the original locality at Chester, Conn., and their analysis afforded the following results. Quantity taken, 775.5 grms. Found—

* Journ. Acad. Nat. Sci. Phil., iii. p. 375.

Silica	0.292	=	37.653	per cent.
Alumina	0.484		62.411	...
	<u>0.776</u>		<u>100.064</u>	

2 atoms Silica	1154.62	=	SiO ³	Required.
3 atoms Alumina	1927.00	=	AlO ³	37.47
	<u>3081.62</u>			<u>62.53</u>
				100.00

This result gives then exactly the formula of kyanite, viz. 2AlO³, 3SiO³. The analyses of Staff and Norton give also the same result*.

We can therefore have no longer any hesitation in referring Sillimanite to kyanite, as originally suggested by Haidinger†.

Bucholzite is a name given by Brandes to a silicate of alumina from Tyrol, which occurs in compact masses of a finely fibrous structure and hardness equal to kyanite. Thomson has also analysed a mineral from Chester County, Pennsylvania, well-known to collectors, and has referred it to *Bucholzite*‡. Being in possession of authentic specimens of the Chester mineral, I have analysed it with the following result. Quantity taken, 0.561 grm. Found—

Silica	0.1925	=	34.31	per cent.	Another sample.
Alumina	0.3615		64.43	...	35.96
Magnesia	0.0028		0.52	...	
Manganese	trace		trace	...	
	<u>0.5568</u>		<u>99.26</u>		

This also will give us the same formula as kyanite. The mineral being less pure than Sillimanite, cannot be expected to furnish results as accurate as the former analysis. Prof. Shepard in his System expresses the opinion that *Bucholzite* and Sillimanite were the same species.

There is also found at Brandywine Springs, Delaware, a mineral which has been extensively circulated under the name of both *Bucholzite* and *fibrolite*. A specimen from this locality

* In Prof. Norton's analysis, which was made in Yale College Laboratory, the excess of 2.73 was owing undoubtedly to aluminate of potash which remained with the alumina after separating the peroxide of iron by caustic potash. Subtracting this sum from the sum of alumina and peroxide of iron, we have 62.30 per cent. alumina and peroxide of iron, which is almost exactly the quantity required by theory, and I have corrected the analysis accordingly with the consent of Prof. Norton. That analysis was made on the Sillimanite from Fairfield, New York.

† In his translation of Mohs, vol. iii. 154.

‡ Erdmann appears also to have made his analysis on the mineral from the same locality.

furnished me the following results, viz. quantity taken, 1·0675 grm. Found—

Silica . . .	0·386	=	36·159	per cent.
Alumina . . .	0·679		63·525	...
	1·065		99·684	...

This is evidently identical with kyanite. Minute traces of iron and manganese, which are found in both the above, are regarded as of no importance in the result, being mere impurities*.

Fibrolite of Bournon.—This mineral was first distinguished by Count Bournon, who detected it among the associated minerals of corundum from India and from China. The name has reference to its fibrous character. It was analysed by Chevenix, who found—

Silica	38·00
Alumina	58·25
	96·25

Even upon so imperfect an analysis, there has been no hesitation with most writers in referring it to kyanite. Having a specimen of this mineral from Count Bournon at my disposal, I have analysed it†. It yielded on 0·427 grm. taken—

Silica	0·1551	=	36·309	per cent.
Alumina	0·2665		62·415	...
Magnesia	0·0030		0·702	...
	0·4246		99·426	

The results just given leave it no longer possible for us to separate Sillimanite, Bucholite, and fibrolite from kyanite. The hardness of Sillimanite proves also to possess the same inequality on different faces which is found in kyanite. The cleavage face is much softer than the angle or side of the prism, so as

* It may be objected to the conclusion that Bucholite is identical with kyanite that I have not analysed a specimen of the original mineral. This I should have done could I have procured one in time for my present purpose. The Chester mineral here analysed was received by Baron Lederer from Dr. Nuttall, and so far as I can learn, no one questions that the mineral from that locality corresponds entirely with the Bucholite of Brändes. I am convinced that those chemists who have obtained so high a per-centage of silica in their analyses of *disthene* minerals, had not taken the precaution to employ the aid of caustic potash, added to the assay during fusion, as recommended by Berzelius; and that if they had re-analysed their silica they would invariably in cases where the amount exceeded 38 per cent., have found in it a portion of alumina.

† The specimen referred to was taken from the collection of Col. Gibbs (now in Yale College), and was received by him from Count Bournon in a large collection of gems which this gentleman furnished to Col. Gibbs.

to be easily scratched with a sharp point of hard steel. The crystalline forms of Sillimanite and kyanite are also identical; the one being derived by the simplest modification from the other. The cleavage in both is in the orthodiagonal.

It may be worthy of remark that "Andalusite" has the same chemical constitution as kyanite, but belongs to the right rhombic form, while kyanite is oblique. Doubtless a case of dimorphism, and perhaps the same may be said with truth of staurolite.

My pupil, Mr. George J. Brush, afforded me essential aid in the foregoing investigation.

V. ON THE BOLTONITE OF SHEPARD, AND THOMSON'S BISILICATE OF MAGNESIA.

The mineral named Boltonite by Prof. Shepard*, is found at Bolton in Massachusetts, in a lime quarry, disseminated in irregular masses, seldom showing any traces of crystalline form. The description of Prof. Shepard is quoted below †.

The changes of colour are peculiar; and often the same mass, which is dark greenish-gray on one end, will have turned light yellow on the other ‡. Hardness, 5.50; specific gravity, 3.008—the same on two specimens, one dark and one light.

This mineral, when first found, was called *Pyralloolite*, and is now so labeled in some old collections. Baron Lederer's cabinet of American minerals, now in the Yale College collections, contains eight or ten specimens of this mineral from Bolton, under the name *Pyralloolite*, which were received, as the catalogue indicates, from Robinson, Shepard, Nuttall, Boyd, and other of the early cultivators of American mineralogy.

In his remarks on this mineral, Prof. Shepard says, it is believed to be identical with the substance described by Dr. Thomson§ under the name of "bisilicate of magnesia;" and accordingly the analysis of Dr. Thomson is quoted under

* Shepard's Treatise on Mineralogy, Newhaven, 1835, vol. i. p. 78.

† Prof. Shepard's description is as follows—"Massive, composition granular: individuals large, cleavage in one direction pretty distinct, in two others oblique to the first, indistinct, but affording indications of a doubly oblique prism, fracture uneven or small conchoidal. Lustre vitreous. Colour bluish gray, yellowish gray, wax yellow to yellowish white. The darker colours change to yellow on exposure to the weather. Hardness, 5.0-6.0. Gravity 2.8-2.9."

‡ Mr. Saemann of Berlin, Prussia, in a paper read before the Am. Assoc. for the Promotion of Science at Cambridge, attributes the change of colour in boltonite to minute grains of magnetic iron found disseminated in the substance of the crystals, which, undergoing change by exposure, leave the mineral of a lighter colour than it was when fresh.

§ Am. Lyc. Nat. Hist., New York, vol. iii. p. 50.

“Boltonite,” as giving the supposed chemical constitution of this substance.

It will presently be shown that there is every probability that Dr. Thomson applied the name bisilicate of magnesia to another substance; and that the boltonite of Prof. Shepard is not the substance which he analysed.

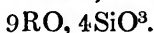
Having received specimens of boltonite from Mr. Saemann, a very intelligent and discriminating mineralogist from Berlin, I was induced to undertake an analysis of it, which gave me the following results. The specimen analysed was the yellow variety. 0.5753 grm. of substance gave—

Silica	46.062	=	...	Oxygen.	23.23	=	8
Alumina	5.667		...		2.64		1
Magnesia	38.149		14.76	} = 17.14	6		
Protoxide of iron	8.632		1.95				
Lime	1.516		0.43				
		100.026						

Formula $8\text{SiO}^3 \cdot 1\text{AlO}^3 \cdot 18\text{MgO} = 2\text{RO}, (\text{SiO}^3, \text{AlO}^3), \text{ or } 2(\text{MgO}, \text{CaO}, \text{FeO}), (\text{SiO}^3, \text{AlO}^3).$

8 atoms Silica	= 370.08	=	46.556	per cent.
1 atom Alumina	51.47		6.372	...
18 atoms Magnesia	372.66		47.072	...
		794.91		100.000	

If we consider the alumina as not an essential constituent of the mineral instead of replacing a part of the silica (a view which I am not disposed to take), then we shall have a silicate of magnesia and the other bases, whose formula will be



Referring to Thomson's analysis and description of his bisilicate of magnesia, we read (*loc. cit.* p. 50) that the mineral received by him from Mr. Nuttall (from Bolton, Massachusetts) bears so much resemblance to the picrosmine of Haidinger, both in character and composition, “that he strongly suspects the two things to be mere varieties. The mineral is white, with a shade of green; powder white. *It consists of a congeries of prismatic crystals, very irregularly disposed, and involved in each other. Lustre glassy; transparent on the edges.*” The analysis gave—

Silica	56.64
Magnesia	36.52
Alumina	6.07
Peroxide of iron	2.46
		101.69

This analysis must certainly refer to another mineral than boltonite. The description certainly does not compare at all with that of boltonite, which cannot be said to "consist of a series of prismatic crystals," with a glassy lustre and faint green colour. Nor is it white. In searching among the minerals from Bolton, in the cabinet of Baron Lederer, for something corresponding with Thomson's description, I found one from that locality marked "Picrosmine?" "Actynolite?" This mineral answers the description of Thomson, quoted above, as nearly as anything could; and is undoubtedly the same thing which he received from Mr. Nuttall, and examined with the above results. Nothing else occurs at the locality at all resembling the mineral which is described by Dr. Thomson. A qualitative analysis of this specimen gave silica, magnesia, alumina, peroxide of iron, manganese, but no lime or water. These are the constituents of a hornblende, and this specimen is undoubtedly such—variety actinolite*.

If the foregoing conclusions are correct, it would appear that boltonite and "bisilicate of magnesia" are not the same mineral as described by Prof. Shepard.

The formula for boltonite is that of a salt not before described, while that deducible from Thomson's analysis, corresponds as accurately as we could expect with common hornblende.

I am happy therefore to be able to re-establish boltonite as a species on good grounds.

VI. ON NUTTALLITE.

Nuttallite was established as a species by Mr. Brooke †, on general physical grounds, principally of hardness and colour, and a slight departure from the usual angles of scapolite. It was analysed by Thomson ‡, who found for it a constitution so different from scapolite, that it has been regarded as a distinct species by many mineralogists, and is so placed by Nicol in his Manual just published. I was induced to make a new analysis to decide the doubt regarding its true constitution. The mineral is partially decomposed by strong hydro-

* I am altogether at a loss to understand what Dr. Thomson intends, when he says in his memoir before quoted, that the analysis here given corresponds to the constitution of a "bisilicate of magnesia." For

2 atoms SiO ³	=	92.52	= per cent.	81.72
1 atom MgO	=	20.70		18.28
		113.22		100.00

This result is entirely different from his analysis.

† Ann. of Philos., xli. p. 366.

‡ New York Lyceum of Natural History, vol. iii. p. 82.

chloric acid with heat, but it is not thus possible to obtain a complete analysis. The mineral is found at Bolton, Mass., in a white cleavable limestone with black augite. Having a good specimen, I requested Mr. Ludwig Stadtmuller, one of our pupils, to undertake the analysis. The following are the results confirmed by several trials; the alkaline constituents being determined by fusion with carbonate of baryta.

Silica	45.791
Alumina	30.107
Peroxide of iron .	1.861
Lime	17.406
Potash	3.486
Soda	} . . traces.
Manganese	
Water	1.630
	<hr/> 100.281

It is obvious from simple inspection, that this analysis corresponds exactly with scapolite, and we have no hesitation in referring Nuttallite to scapolite.

LVII. Intelligence and Miscellaneous Articles.

ON THE STATE IN WHICH ARSENIC EXISTS IN THE DEPOSIT
FROM MINERAL WATERS. BY M. J. L. LASSAIGNE.

SINCE the discovery of the presence of arsenic in the deposits from certain chalybeate mineral waters, it has been asked whether the poisonous properties of this substance are not neutralized by the state in which it is found. No experiment having been yet undertaken on this subject, the author, at the request of M. Chevallier, has made several experiments. The object in undertaking them was to determine the proportion of arsenic contained, in what state of combination it existed, and the nature of the action which these arseniferous deposits exerted on the animal œconomy.

The first experiment was made on the deposit from the waters of Wattviller (Haut Rhin). In order to ascertain the quantity of arsenic, a portion of the residue was treated with nitro-hydrochloric acid, to convert all the arsenic which it might contain into arseniate of iron.

This residue, washed with distilled water, was calcined in a silver crucible with twice its weight of hydrate of potash; the product of this calcination was treated with hot distilled water, and the liquid was filtered to separate the insoluble deposit; the filtered liquid was supersaturated with nitric acid, and the solution evaporated to dryness. The residue, redissolved by distilled water, gave a solution to which was added solution of acetate of lead; this produced a white flocculent precipitate which was collected on a weighed filter. This

precipitate was recognized as arseniate of lead,—1st, by the alliacious odour which it evolved when calcined on charcoal by the blowpipe; 2ndly, by the brick-red colour which it immediately yielded by moistening it with a neutral solution of nitrate of silver. From the weight of this arseniate of lead, the quantity of arsenic contained in the deposit from the waters of Wattviller, was deduced. This experiment showed that 100 parts of the deposit, previously treated with nitro-hydrochloric acid and afterwards dried, yielded 4.42 of arsenic acid, containing 2.8 of metallic arsenic.

After having determined the proportion of arsenic, a direct experiment was attempted to determine the effect of this deposit on the animal œconomy. This experiment was made with a fresh quantity of the same deposit, furnished by M. Chevallier.

Forty grammes of this deposit, divided into two portions, were forcibly given to a middle-sized young dog. Each dose of 20 grammes was diffused through a decilitre and a half of slightly viscid honeyed water, and gradually introduced into the throat of the animal. This liquid, which was swallowed without any apparent disgust, occasioned neither vomiting nor uneasiness, for on offering bread to the animal three quarters of an hour after swallowing, it was readily eaten. The second dose was administered in the same manner, and afterwards the animal eat about three ounces of brown bread. No alteration of appearance could be detected by an attentive examination of the animal, during six hours from the commencement of the experiment, and no difficulty was observable in its digestion, even in twenty-four hours. After this time the animal was restored to his usual diet, and there was no apparent alteration in his health.

The following conclusions may be fairly drawn from this first experiment:—

1st. In the natural deposits of the mineral waters of Wattviller, arsenic exists to the amount of 2.8 per cent.

2ndly. A portion of these deposits, representing 1.76 gr. of arsenic acid, or 1.14 gr. of arsenic, produced no effect upon the health of a dog.

3rdly. This non-action shows that the poisonous property of the arsenic contained in these deposits, is destroyed by its combination with peroxide of iron.

4thly. This result confirms what experiment has already shown, that peroxide of iron, by combining with arsenious and arsenic acid, destroys their poisonous properties, and consequently becomes an antidote for them, as proved long since by the direct experiments of MM. Bunsen and Berthold Font.

A second chemical experiment was made upon a certain quantity of the deposits of the waters of Royat (Puy-de-Dôme), sent to the author by M. Chevallier; in this deposit, in which M. Chevallier and M. Gobley had ascertained the presence of arsenic, it was found to amount to only $\frac{3}{1000}$ dths, and in the state of arseniate.

The last conclusion was deduced from the presence of arseniate of iron and of lime, which was obtained from the deposit in hydro-

chloric acid and treatment of the dry residue with alcohol. The small quantity of pulverulent matter, of a chamois yellow colour, which remained insoluble, consisted of arsenic acid, peroxide of iron and lime.

The author will not undertake to decide that the acid is combined with these two bases in the deposit; it being possible that a part of the subarseniate of iron had been decomposed during the operation by carbonate of lime, of which the deposit from the waters of Royat contains a large quantity.

Two hundred grammes of this deposit were divided into six portions of 33 grammes. Each dose was diffused through honeyed water and given every two hours to the same dog; three doses were given in one day and three others the next; the animal showed no peculiarity during the experiments, and continues healthy and lively. — *Journ. de Chém. Méd.*, Septembre 1849.

EASY MODE OF MEASURING SOLAR OBJECTS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I am not aware whether the expedient is generally known of measuring the solar spots by placing a graduated glass scale on the diaphragm of the eye-piece and casting the sun's image upon a white ground. The divisions of the micrometer become thus more palpably distinguished, and the excess covered by the spots is more accurately estimated by the eye than when looking direct through the telescope. In this way also an angle can be measured in any direction by merely turning the eye-piece round on its axis. It greatly saves the eye-sight, and is serviceable when only brief glimpses are to be obtained and expedition is required. Although perfect accuracy may not be attained, it may serve at least for general purposes.

Should the suggestion seem to you worth noticing, I beg to leave it to your disposal.

I am, Gentlemen,

Edinburgh, Nov. 12, 1849.

Your obedient Servant,

W. PRINGLE.

NATURAL SOURCES AND NEW MODE OF PREPARING SULPHURIC ACID. BY M. C. BLONDEAU.

Abundant sources of sulphuric acid exist in Nature. M. Bous-singault has described several acid waters in America, and particularly the Rio-Vinagre or Pasiambo, of which 1000 parts contain 2 parts of sulphuric acid. According to M. Boussingault's estimate, the Pasiambo supplies 38610 kilogrammes of sulphuric acid in 24 hours, and this quantity is much exceeded by the discovery made in the Paramo de Riuz by M. Degenhart, the water there containing, according to M. Lewy's analysis, three times as much sulphuric acid as the Pasiambo. Whence come these enormous quantities of the acid? what are the processes which nature employs in their formation?

The author states, that a phænomenon which he observed in the department of Aveyron, gave him an opportunity of describing the natural formation of an acid so much employed in the arts, and which placed him in a condition in the localities which he has examined, of readily manufacturing sulphuric acid, without having recourse to the complicated processes generally employed.

In the coal-measures of Aveyron, and particularly in the environs of Cransac (arrondissement de Villefranche), the spontaneous combustion of the soil is observed to occur, which is evidenced by the disengagement of gas and vapours, which at a distance resemble a small volcano. On approaching the place where this combustion occurs, it is evident that the earth has been mined, and large crevices are discovered from time to time, from which there is emitted much aqueous vapour and acid fumes. At the edges of these fissures the heat becomes intolerable, and surprise ceases to be excited that the effects of this heat, combined with the action of acid gases, should have modified so completely the places in which these chemical actions occur.

In some spots of the burning mountain, there occur enormous rocks formed of conglomerates, which, having undergone the action of fire, are completely changed in appearance, and are united by a cement, which owing to the action of heat has a brick-red colour.

The surface of the burning mountain consisted of grits, schists and argills; these substances have assumed the appearance of chalcedonies, jaspers, enamels, glass and bricks, and sometimes even the cavernous appearance of volcanic stones. The aggregations which these substances have formed with argill have in some cases acquired the hardness of the most compact stones. The soil, gradually mined by the chemical agency occurring within it, eventually sinks, occasioning the formation of foundries, which, by their conical form, resemble in some degree the craters of volcanos; it is through these vents that columns of vapour are disengaged, which sometimes rise to a great height in the air, and are at other times dispersed by the wind in the valleys.

In these places a number of saline concretions, efflorescences, crystals of sulphur and hydrochlorate of ammonia are met with; these products have been converted to useful purposes, and dissolved in rain-water, they constitute the mineral waters frequently employed in the locality now described.

The causes of the phænomena become evident to any one who has ascertained the presence of sulphuret of iron, which occurs abundantly in the various strata of coal country which constitute this locality.

This sulphuret, in contact with water and with atmospheric air, burns and gives rise to sulphurous acid gas, which is converted into sulphuric acid by the influence of air and of bases, such as alumina and oxide of iron. The sulphates of iron and alumina which form under these circumstances are decomposed by the action of heat, and sulphuric acid is set free.

The temperature resulting from these different reactions is some-

times sufficiently high to occasion the combustion of the coal-beds which are near the surface, and the products of the combustion of the coal are added to the vapour of water and of sulphuric acid, and thus increase the grandeur of the phænomenon. The sulphuric acid which arises under the conditions described, exerts a very energetic action on the mineral and organic substances which it meets with in its passage; the trunks of the trees which occur in the neighbourhood of the burning mountain are covered with the black colour of substances which have been immersed in sulphuric acid. Mineral substances are also strongly acted upon by this powerful acid, which simultaneously attacks silica, alumina, lime, oxide of iron, the earths and alkalis which enter into the composition of rocks, and eventually sulphates are produced, among which is the double sulphate of potash and alumina (alum) in sufficient quantity to be useful.

The author analysed the efflorescences collected on the burning mountain of Cransac. These efflorescences were white, strongly acid, reddened tincture of litmus, and attracted moisture from the air. After drying *in vacuo* by the air-pump, 50 grammes were dissolved in a litre of distilled water, and the solution was treated as if it had been a common mineral water

The results of the analyses were:—

Sulphate of potash and alumina	24.25
Sulphate of alumina	53.31
Sulphate of magnesia	3.47
Sulphate of manganese	1.35
Sulphate of iron	10.29
Free sulphuric acid	7.33
	<hr/> 100.00

On examining the natural process which gives rise to the large quantities of sulphuric acid, occurring not only combined with bases, but also uncombined, it occurred to M. Blondeau to examine whether under similar conditions sulphuric acid might not be immediately produced from sulphurous acid gas.

For this purpose some argillaceous sand was put into a porcelain tube, one of the ends of which communicated with two vessels, from one of which sulphurous acid, and from the other vapour of water was disengaged, and at the same time air was passed into the interior of the apparatus by means of a gasometer. At the other end of the porcelain tube a bent tube was adopted, which was immersed in water in a two-necked bottle, to one of which was fixed a disengaging tube. The apparatus thus arranged, the porcelain tube was surrounded with burning charcoal, so as to heat it to dull redness, and the sulphurous acid gas, air and the vapour of water were slowly passed into it. The substance disengaged at the end of the tube was sulphuric acid; taking care to supply an excess of air, but very little sulphurous acid is disengaged, the whole of it being converted into sulphuric acid.

To go from this laboratory experiment to a manufacturing one, sulphurous acid must be produced by the combustion of sulphur or

sulphurets, and the products of the combustion passed into a cylinder of cast-iron strongly heated and containing argillaceous sand, passing into it at the same time excess of the vapour of water. The sulphuric acid will be received at the other end of the cylinder. The author is of opinion that no doubt can be entertained of the superiority of this plan to that which is at present adopted, and that by employing an apparatus thus constructed, sulphuric acid will be procured at a lower price than is at present the case.—*Comptes Rendus*, Oct. 15, 1849.

NOTES ON THE CALIFORNIA GOLD REGION.

BY THE REV. C. S. LYMAN*.

From the western base to the summit of the range of the Sierra Nevada, is a distance generally of a hundred miles, or more. The western slope is broken and precipitous, and through the deep ravines that abound, flow the numerous mountain streams that form the tributaries of the Sacramento and San Joaquin rivers. The gold region is a longitudinal strip or tract from ten to forty miles in width lying about midway, or a little lower, between the base and summit of the range, and extending in length a distance of many hundred miles—active operations being already carried on through an extent of four or five hundred miles at least. The gold mines near San Fernando in a spur of the same range, and which have been known and worked to some extent for many years, are doubtless a part of the same great deposit.

On approaching the gold region from the valley of the Sacramento or San Joaquin, soon after leaving the plain, the attention is arrested by immense quantities of quartz pebbles, slightly rounded, and of the size of walnuts, scattered over the gentle elevations which form the western base of the Snowy Mountains. There is here but little soil—the earth is of a yellowish red colour, and nearly destitute of vegetation. Nearer to the gold deposits the quartz pebbles become larger, and not unfrequently boulders are noticed of considerable size. The quartz is so uniformly associated with the gold, that even the most unscientific explorer would not think of looking for the metal where quartz did not abound. Passing up the mountains it is easy to tell when you leave the region of gold from the sudden disappearance of the quartz. In August of last year, in company with Mr. Douglass and others, I ascended from the “dry diggings,” near the Rio de los Americanos, to within a few miles of the snow, enjoying in the highest degree the sublime scenery presented by lofty and precipitous mountains, separated from each other by dark, deep ravines, and wooded with primæval forests of towering firs and pines. The backbone of this mountain range is granite, the several varieties of which constituted almost the only rock visible in the last few miles of our journey. In descending we passed successively several forms of gneiss and other primitive and transition rocks, till we

* In a Letter to B. Silliman, Esq., dated Puebla de San Josè, March 27, 1849.

reached the slate formation which prevails in this part of the gold district. We penetrated on this occasion some forty or forty-five miles beyond the "dry diggings," and after leaving the quartz twelve or fifteen miles up, scarcely a particle of gold was discovered.

As I have mentioned, the prevailing rock of the gold region near the Rio de los Americanos is slate. There are many varieties of it—some shaly and friable, others hard and massive, somewhat resembling greenstone. The laminæ of the slate beds are nearly perpendicular, and their direction about N.N.W. and S.S.E., or nearly the same as the direction of the range. These slate beds often include dykes or beds of quartz rock several feet in thickness. At the dry diggings above-named, I passed at right angles over the upturned edge of continuous strata of slate a distance of four or five miles; and in the same direction, slate beds occur several miles further on, but I had not the means of knowing that they were a part of the same great deposit.

In some of the richest explorations yet made, the slate formation immediately underlies the stratum of drift or diluvium which contains the gold, and much of the gold is found in the crevices of the slate, the rough edges of the upturned strata forming innumerable receptacles or "*pockets*," as they are called, into which the metal has originally found its way, from its own gravity assisted by aqueous agency. It is this accidental association of the gold with the slate rocks which has caused the statement to be frequently made, even by persons of much general intelligence, that the gold exists in the body of the rock itself, and forms a component part of it, in the same sense that iron pyrites forms a part of the rocks in which it occurs. But I have nowhere seen gold among the slate, except in circumstances where its presence could be accounted for by its introduction from without, a close scrutiny readily discovering some cleft or opening through which it might have entered. The richest of these "*pockets*" are in the bottoms of sharp ravines which seem to have been notched into the body of the slate, and generally in situations where the bottom of the ravine, after descending at a considerable inclination for some distance, becomes more nearly horizontal. Just below a sudden descent or precipice, in the bottom of a dry ravine, gold is often found in the cavities in great abundance. From such a spot Mr. Douglass extracted a pound of gold in a few hours, even after the place had been previously "dug out," as was supposed, and abandoned.

I have noticed in published accounts, many erroneous statements respecting the geological position of the gold. Some have said there is no particular formation in which the gold occurs, but that in different places it is found in different kinds of earth or rock. You will not need to be informed that this is without foundation. So far as I have been able to examine, or can learn from competent witnesses, there is but *one* geological formation with which the gold of the Sierra Nevada is associated and in which it uniformly occurs. This is the stratum of *drift* or *diluvium*, composed of a heterogeneous mixture of clay, sand, gravel and pebbles, and varying in thickness from

a few inches to several feet. Here, as elsewhere, this stratum is neither horizontal nor of uniform slope, but conformed to the varying inclination of the earth's surface, covering the declivities, and even the summits of the hills, as well as the bottoms of the ravines and valleys. Out of this stratum I have nowhere found gold, except where a stream has cut it away and made its contents a part of some alluvial formation of comparatively modern date. The sand-bars of some of the mountain torrents, and the gravelly projections formed at the bendings of the streams, are often extremely rich in metal. A bar in the Rio de los Americanos (at high-water an *island*), about twenty-three miles above New Helvetia (now called Sacramento), and on which some of the earliest explorations were made, is of this character. But where the diluvium has remained undisturbed since the period of its deposition, I am confident no "alluvial" or "stream" gold has been, or will be discovered, except in connection with it. It is evidently as much "part and parcel" of this formation as its associated quartz, greenstone, hornblende, and other pebbles, and whoever will explain the origin of the one, will at the same time elucidate the origin of the other, for one and the same agency unquestionably spread both of them over the surface of the district. What the latest theory of geologists is to account for the dispersion of drift, I am too isolated from the scientific world to know. Quartz is the only substance with which I have seen the gold intimately united, and these compound lumps seem to show clearly that the original *matrix* or *vein-stone* of the metal was a dyke or bed of quartz rock. And we have only to suppose, that when the quartz, with its accompanying rocky strata, was broken up by natural agencies at some former geological epoch, the interspersed or included veins of gold were at the same time reduced to fragments, and these rough and angular fragments subsequently broken and further comminuted and rounded by mutual attrition, to account for the present form and appearance of the gold, and for its constituting a portion of the materials of the drift. But whether these materials with their golden treasure, now occupy the precise geographical position of their parent rocks, or whether they have been transported by aqueous or glacial agencies or both, from some neighbouring or perhaps far distant locality, is a question which future investigations into the geology and physical geography of the region will better elucidate than the imperfect data at present in my possession. I cannot avoid the fancy, however, in connection with the glacio-aqueous theory, that when the continent was wholly or partially submerged, the materials of the diluvium, including the gold, were transported by icebergs from their parent locality, and when at length set free, left to assume their present position on what was then the rocky and uneven bottom of the superincumbent ocean. And we have only to imagine these freighted icebergs stranded by oceanic currents against the partially emerged range of the Sierra Nevada, to account for the great longitudinal extension of the gold region along the western slope of the mountains, while laterally it appears to extend neither above nor below certain definite limits.

The gold of different localities varies very much in size. That from the banks and sand-bars of the rivers, is generally in the form of small flattened scales, and commonly it is found to be finer the lower down you descend the stream. That taken from the bottoms of the dry ravines, which everywhere abound in these mountains, and furnish outlets for the torrents of the rainy season into the principal streams, is mostly of larger size, and occurs both in small particles and also in small lumps and irregular water-worn masses, from the size of wheat kernels to pieces of several ounces or even pounds in weight. The fine gold of these ravines is commonly less worn and flattened than that in the alluvium of the rivers. And the flattened scale-like form of the gold in these latter deposits would seem to be owing to the great malleability of the metal—the stones and pebbles among which the minuter particles and fragments of the original vein of native metal chanced to lie, and by which they were rudely hammered, having performed very effectually the gold-beater's office, and gradually reduced the rough angular particles, on their granite anvils, to the flattened spangles which we now observe. Some of these flakes are often an inch or more in diameter and scarcely thicker than paper. Many specimens bear the distinct impression of the crystalline structure of granite and other rocks; and I have seen several pieces deeply stamped, as with a die, by crystals of quartz, the form of the crystal being as distinctly apparent as the device on a gold eagle fresh from the United States mint.

The black, ferruginous sand, which everywhere accompanies the gold, and which, from its great specific gravity, remains with it in the bowl or machine after the other earthy materials have been removed, varies in fineness with the size of the accompanying gold; that obtained in connection with the fine river gold being of the fineness of writing sand, while that associated with the coarse gold of the ravines is often as large as wheat kernels, or peas, and sometimes of the size of hazel nuts or walnuts. These coarser pieces are fragments of crystals very hard and heavy. I found no specimens with the faces complete, and have not the means of knowing to what species they belong, but suppose them to be magnetic iron. That the fine sand is composed of fragments of the same crystals greatly comminuted, I infer from the regular gradation of the one into the other.

I am not aware that the gold has yet been discovered *in place*, or imbedded in its native matrix. The slates, however, of the gold region, as I have before observed, are often traversed by dykes or beds of quartz rock, and I have examined these in many places for indications of the presence of the metal, but could detect no traces of it. Individuals have asserted that they have found veins of it in the rocks, but they have refused to divulge the place where, inasmuch as they intended to work the veins themselves as soon as the season would permit. Though these statements are of course not impossible nor indeed improbable, I do not consider the fact as established by testimony, since the witnesses are men in whom I place but little confidence.

The amount of gold taken from these mines it is impossible to

estimate, but it has been immense, and the coming season it will doubtless be greater. New and rich deposits are developing every day. Accounts from various points in the mining district, represent the gold as very abundant, more so if possible than last year—individuals even early in the season obtaining often from three to ten or even twenty ounces a day. The diggings on the several forks of the Rio de los Americanos, the Stanislaus, the Tuwalumnes, the Merced, the Mariposa, King's river (Lake Fork on Fremont's new map), and in many other places, are represented as peculiarly rich.

There was one specimen of gold mingled with quartz, found near Stanislaus last autumn, which I had resolved to procure, if possible, for the cabinet of Yale. It was irregular in form, about four inches in diameter, and weighed $5\frac{1}{4}$ pounds avoirdupois. The metal was interspersed in irregular masses through the stone, and as near as I could judge without special investigation, was equivalent to about two pounds troy, perhaps a little more. Other specimens much larger are said to have been found, and one of twenty pounds weight pure, near the Stanislaus; but these I have not seen.—Silliman's *Journal*, November 1849.

COMBINATIONS OF OIL OF TURPENTINE AND WATER.

BY M. H. DEVILLE.

Oil of turpentine and some isomeric compounds have the property of combining with water, to form substances which well deserve the name of *hydrates*, on account of the facility with which this water may be separated from them. But some singular reactions which occur entitle them to be regarded as compounds of a very peculiar order, and which are without analogy in the history of products of the same kind.

These bodies lose a part of their combined water by the action of heat or exposure to a dry vacuum, and they regain it by exposure to a moist atmosphere.

The action of reagents seems to indicate, at least with oil of turpentine, that the hydrate does not contain the primitive oil in combination. The compound of camphor, obtained by means of hydrate of turpentine and hydrochloric acid, is a proof of this. It will also be seen, that this property has allowed of the conversion of oil of turpentine into oil of lemons, or at any rate into a substance which has all its chemical properties and characteristic odour.

It has long been known that the oils of turpentine and lemon sometimes deposit crystals which, as regards composition, differ from the oil only by the presence of six equivalents of water. These are the results to which the only good analyses that have been performed, lead. They are those of MM. Dumas and Peligot, who, in point of fact, found the formula of the crystalline bodies of oil of turpentine and cardamom, &c. to be $C^{20}H^{22}O^6 = C^{20}H^{16}, H^6O^6$.

Some years since M. Wiggers observed that in certain veterinary medicines formed of a mixture of alcohol, nitric acid, and oil of turpentine, there was deposited a considerable quantity of a crystallized

substance, which possessed the composition of the hydrates analysed by MM. Dumas and Peligot.

M. Deville continued the researches of M. Wiggers, and found that, to obtain the hydrate of oil of turpentine in a short time, the most convenient substances and proportions were, 4 litres of commercial oil of turpentine, 3 litres of alcohol at 85°, and 1 litre of common nitric acid. At the expiration of a month or six weeks 250 grammes of very pure crystals were obtained, and eventually more than a kilogramme was gradually deposited. The oils of lemon and bergamot yield the same results when similarly treated. Oil of copaiba with nitric alcohol acquires much colour, and after a long time yields so small a quantity of crystals, that they could not be analysed.

If a mixture be made of oil of turpentine and crystallizable acetic acid, no effect is produced even in several years; but if a few drops of nitric acid be added to the mixture, solution takes place in a few days, and crystals are soon deposited. On putting the liquid *in vacuo* over a vessel full of potash, and another full of sulphuric acid, the vapour of water, of nitric and acetic acids, and of oil of turpentine is absorbed, and there remains a blackish paste, from which, by means of alcohol, there may be extracted crystals of the same form and composition as those obtained with nitric alcohol.

The action of the nitric acid in the combination of water with the oils it is very difficult to determine. It is to be remarked that the acid does not increase the solubility of the oil in the alcohol, but on the contrary, diminishes it. Neither dilute alcohol, alcohol almost absolute, nor alcohol acidified with nitric acid, had any effect on oil of turpentine after having been mixed with it for several years. Pure water and these oils combine, however, though very slowly, and in small quantity but regularly, in vessels in which the oils are kept impregnated with moisture. The crystals thus produced, although possessing the same composition as those deposited from nitric alcohol, differ considerably from them in form, as will be seen hereafter. It is also to be remarked, that the hydrate of oil of turpentine, which is formed in a mixture of acetic and nitric acid, assumes a form which is sensibly different from that which it has after solution in and crystallization from alcohol.

The author states that he should have been curious to examine these various compounds which so resemble each other, and which are perhaps dimorphous and chemically identical. Unfortunately it is very difficult to procure the hydrate formed accidentally in old oils, that is to say, the product analysed by MM. Dumas and Peligot, so that all hope of obtaining it in sufficient quantity, even for an imperfect examination, was relinquished.

Hydrate of oil of turpentine is one of the most beautiful substances obtainable, on account of its size, perfection, limpidness and splendour of its crystals, which are right prisms with rectangular bases.

This substance exerts no action on the plane of polarization; it fuses from 217° to 221°, losing a little water; when exposed to a

higher temperature its composition changes. When it has been melted it does not perfectly solidify on cooling, but remains soft, and may be drawn into threads at common temperatures. It is at first transparent, and then after some time it becomes a mass of radiating crystals. At 50° Fahr. 100 parts of alcohol of 85° dissolve 14.49. Its composition, as well as that of the hydrates of oil of lemon and bergamot, is as follows:—

	Experiment			
	I.	II.	III.	
Carbon.....	63.2	62.9	63.0	Calculation ($C^{20}H^{22}O^6$)
Hydrogen ...	11.7	11.7	11.7	
Oxygen	25.1	25.4	25.3	
	<hr/> 100.0	<hr/> 100.0	<hr/> 100.0	

When hydrate of oil of turpentine is heated to a higher temperature than that at which it melts, it is very rapidly decomposed into water and a new hydrate, not containing more than two equivalents of water, and which almost entirely evaporates; the residue is inappreciable.

It will be remembered that MM. Blanchet and Sell have found and analysed a substance obtained from oil of turpentine, to which they have assigned the formula $C^{20}H^{16}H^2O^2$. In the nomenclature adapted to these series of compounds, the name of *monohydrate* of oil of turpentine ought to be given, as well as to the product derived from distilling the hydrate obtained from the action of nitric alcohol on oil of turpentine; M. Deville gives the name of *perhydrate* to the crystals collected in moist oil of turpentine or treated with nitric alcohol.

Heat is not the only agent which is capable of converting the terhydrate of oil of turpentine into bihydrate; the same effect is produced by a dry vacuum. Moreover, the terhydrate is reproduced, when, after having lost, by either mode, 2 equivalents of water, it is left exposed for some time to moist air. This singular reaction is probably unparalleled in organic chemistry: it is surprising to observe a substance which is totally insoluble in water, like the bihydrate of oil of turpentine, absorb water from a moist atmosphere, as is shown by the result of quantitative analyses performed with care.

The following are the results of an analysis of the bihydrate, recently prepared by means of many distillations:—

	Experiment.	Calculation. ($C^{20}H^{20}O^4$)
Carbon.....	69.4	69.76
Hydrogen ...	11.8	11.63
Oxygen	18.8	18.61
	<hr/> 100.0	<hr/> 100.00

The density of the vapour confirmed this formula; by experiment it was 6.257, by calculation 6.01; the bihydrate possesses no acid reaction; when heated with potash, it is volatilized and does not combine with it, although its composition would lead to the con-

clusion that it would have the properties of a fatty acid ; it boils fixedly at 282° Fahr. ; it is volatile without residue.

When oil of turpentine is treated with nitric alcohol, another substance is obtained which may be considered as a liquid hydrate. After remaining mixed for several years, these substances do not dissolve, and the oil of turpentine is not entirely metamorphosed. On heating the viscid and coloured oil which floats on the nitric alcohol to 428° Fahr., water first comes over, then oil of turpentine, afterwards a peculiar liquid, into the composition of which, judging from the results of analysis, the elements of water enter ; it is probably only impure liquid bihydrate of oil of turpentine ; it yielded by analysis,—

Carbon.....	76.4
Hydrogen	11.6
Oxygen	12.0

100.0

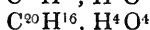
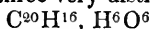
By analogy it ought to be admitted that a liquid terhydrate cannot exist at 292° Fahr. If it be supposed that an oil be present in this product representing a liquid hydrate, its composition must be,

C^{20}	69.8
H^{20}	11.6
O^4	18.6

100.0

This formula is that of the solid bihydrate of oil of turpentine.

It appears from the above detailed experiments, that oil of turpentine forms with water three very distinct compounds :—



The two first may be converted into each other at pleasure, since a dry vacuum takes away 2 equivalents of water from the terhydrate, and moist air restores 2 equivalents of water to the bihydrate.—*Ann. de Ch. et de Phys.* Septembre 1849.

ACTION OF PHOSPHORIC ACID ON THE HYDRATES OF OIL OF TURPENTINE. BY M. H. DEVILLE.

When the bihydrate or terhydrate of oil of turpentine is treated with anhydrous phosphoric acid, a colourless oil is obtained, which by distillation is separated into two other oils of different volatility : one is tereben, easily recognized by its odour, fluidity and composition. It gave by analysis—

	Experiment.	Calculation. ($C^{20}H^{16}$)
Carbon.....	88.1	88.24
Hydrogen.....	11.9	11.76
	<hr/> 100.0	<hr/> 100.00

The other oil is viscid, boils at a higher temperature, and is more dense than the above ; it possesses characteristic dichroism, sometimes appearing blue and at other times colourless : it is colophen.

Analysis gave—

	Experiment.	Calculation. (C ⁴⁰ H ³²)
Carbon.....	88.1	81.24
Hydrogen.....	11.8	11.76
	<hr/> 99.9	<hr/> 100.00

Oil of turpentine acts exactly in the same way with anhydrous phosphoric acid as the hydrates, being like them converted into tereben and colophen.—*Ann. de Ch. et de Phys.*, Septembre 1849.

ON THE INFLUENCE OF BORACIC ACID ON VITRIFICATION.

M. Maes, manufacturer of flint-glass, has, conjointly with M. Clemandot, long paid attention to the above-named subject. The principal results hitherto obtained are:—1st, borosilicate of potash and lime; 2ndly, borosilicate of potash and zinc; 3rdly, borosilicate of potash and barytes; 4thly, borosilicate of soda and zinc.

The borosilicate of potash and lime was formed with the intention of producing in close vessels with coal furnaces, the best imitations of Bohemian glass. In the *Compte Rendu de l'Exposition Autrichienne*, 1845, published by M. Peligot, it appears, that in order to make the purest and most durable glass in Bohemia, they use with 100 parts of silica, 12 parts of unslaked lime, and only 28 parts of carbonate of potash. From this we must conclude that the glass is better the less potash and the more lime it contains.

The above proportions yield a glass which is infusible in the furnaces employed by M. Maes. The addition of a few hundredths of boracic acid is sufficient to occasion fusion, and the resulting product possesses all the limpidity, splendour and hardness which can be desired.

This first experiment naturally suggested the advantage which might be derived from the solvent power of boracic acid so as to introduce bases into glass which had not hitherto been employed, as borosilicate of potash and zinc, and that of potash and barytes. The borosilicate of potash and zinc appeared to impart all the qualities of a pure and durable glass. As to the borosilicate of potash and barytes, it was prepared from native carbonate of barytes, contaminated with sulphate of barytes and a ferruginous gangue. If then it be less colourless than the zinc glass, the colour is certainly accidental: on again making it with pure carbonate, this imperfection would unquestionably disappear.

The beauty of borosilicate of potash and zinc led to the comparative trial of borosilicate of soda and zinc: this, although inferior to the potash, incontestably excelled all the soda glasses compared with it.

To recapitulate: the borosilicates are chiefly remarkable for their transparency and hardness. They derive these important qualities from reducing considerably the potash and soda which almost always are in excess in common glass; and every one knows that glass which is too alkaline, is cloudy, soft and hygrometric.

These observations, in the opinion of the author, warrant the conclusion, that boracic acid must before long contribute to the perfection of glass for optical purposes, and M. Maes proposes to prepare borosilicates of great density, with lead, bismuth, &c., besides barytes.—*Comptes Rendus*, Oct. 22, 1849.

METEORS.

On the 24th, about 8½ P.M., I saw two fine meteors in a north-east direction, one about ten minutes after the other. The former seemed to burst like a sky-rocket and fall a little way; the latter to shoot in a north direction and fall to the horizon in pieces of blue colour. On the 30th, a little before 7 P.M., a very splendid one was seen in this parish and also in Kirkwall, which is nearly twenty miles off. Here it appeared first near the zenith and travelled westward.—*Rev. C. Clouston, Sandwick Manse, Orkney.*

METEOROLOGICAL OBSERVATIONS FOR OCT. 1849.

Chiswick.—October 1. Drizzly: overcast. 2. Cloudy. 3. Constant heavy rain. 4. Heavy rain in the morning: showery. 5. Clear: fine: overcast. 6. Fine: rain at night. 7. Hazy: cloudy: rain. 8. Cloudy and cold: clear: slight frost at night. 9. Clear: very fine: frosty at night. 10. Dense fog: very fine: clear. 11. Cloudy: clear. 12. Cloudy and cold: clear. 13. Rain. 14. Cloudy and cold. 15. Fine: clear at night. 16. Foggy: hazy: overcast. 17. Cloudy and fine. 18. Very fine: clear at night. 19. Slight fog: exceedingly fine: clear. 20. Very fine. 21. Hazy: clouds: rain. 22. Foggy: fine. 23. Cloudy and fine. 24. Overcast: fine. 25. Showery. 26. Cloudy. 27. Drizzly. 28. Overcast: very fine. 29. Foggy: exceedingly fine. 30, 31. Very fine.

Mean temperature of the month 49°·58

Mean temperature of Oct. 1848 49°·59

Mean temperature of Oct. for the last twenty-three years 50°·51

Average amount of rain in October 2·58 inches.

Applegarth Manse, Dumfries-shire.—Oct. 1. Fair, but damp and raw. 2. Frost, hard: clear and fine. 3. Frost: rain P.M.: snow on the hills. 4. Frost A.M. 5. Frost A.M.: shower P.M. 6. Frost still harder: thermometer 24½°. 7. Frost milder: wind high. 8. Cold, but little frost: fine. 9. Frost hard again: shower P.M. 10. Frost hard: fine day. 11. Frost slight: a few drops. 12–14. Frost slight: clear and fine. 15. Frost: cloudy. 16. Little or no frost: cloudy. 17. Frost hard again: rain P.M. 18. Mild: rain: cleared P.M. 19. Mild: cloudy: threatening rain. 20. Fair: variable: high wind P.M. 21. Showers, but mild. 22. Slight frost A.M.: heavy rain P.M. 23. Mild: rain during night: rain P.M. 24. Rain all day: flood. 25. Rain all day: fog: flood. 26. Fine: one shower: cleared. 27. Rain again: thick weather. 28. Fine: clear. 29. Frost A.M.: fine: cloudy P.M. 30. Dark and cloudy: rain P.M. 31. Showery all day: mild.

Mean temperature of the month 44°·0

Mean temperature of Oct. 1848 46°·5

Mean temperature of Oct. for the last twenty-five years ... 46°·6

Mean rain in October 3·25 inches.

Rain, number of days in which it fell, 15. Average rain in Oct. for twenty years 3·56 „

Sandwick Manse, Orkney.—Oct. 1. Bright: hail-showers. 2. Sleet-showers. 3. Clear: frost. 4. Showers: clear: frost. 5. Clear: showers. 6. Bright: clear: frost. 7. Clear: aurora. 8. Clear: showers. 9. Sleet-showers: clear. 10. Fine: very clear: aurora. 11. Frost: fine: very clear: aurora. 12. Fine: very clear. 13. Cloudy: aurora. 14. Bright: clear: aurora. 15. Showers. 16. Cloudy. 17. Bright: showers: cloudy. 18. Drizzle: cloudy. 19. Bright: showers: cloudy. 20. Showers: cloudy. 21. Cloudy: showers. 22. Cloudy: fine: showers. 23. Showers: clear: aurora. 24. Fine: aurora. 25. Rain: fine: aurora. 26. Showers: clear. 27. Bright: showers. 28. Bright: showers: clear. 29. Rain: cloudy. 30. Cloudy: showers: aurora. 31. Bright: fine;

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

Days of Month.	Barometer.				Thermometer.				Wind.			Rain.		
	Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.	
	Max.	Min.	8½ a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	8½ p.m.
1849. Oct.														
1.	29.598	29.427		29.59	29.61	29.62	57	45						
2.	29.723	29.686		29.60	29.60	29.57	55	38						
3.	29.520	29.206		29.42	29.54	29.45	61	45						
4.	29.479	29.027		29.18	29.33	29.35	53	33						
5.	29.749	29.682		29.46	29.50	29.49	56	37						
6.	29.770	29.654		29.62	29.62	29.79	55	44						
7.	29.296	29.128		29.51	29.60	29.86	64	49						
8.	29.916	29.546		29.75	29.83	29.94	54	29						
9.	30.018	29.951		29.86	29.80	29.69	56	28						
10.	29.877	29.692		29.79	29.70	29.90	56	31						
11.	29.526	29.475		29.63	29.68	29.89	54	42						
12.	29.614	29.513		29.73	29.81	29.98	53	39						
13.	29.869	29.690		29.91	30.03	30.14	48	33						
14.	30.074	29.987		30.18	30.21	30.28	51	39						
15.	30.074	30.055		30.20	30.24	30.28	50	34						
16.	30.057	29.981		30.00	29.80	29.91	53	37						
17.	29.955	29.888		29.58	29.53	29.50	66	52						
18.	30.164	30.130		29.79	29.88	29.52	67	44						
19.	30.060	29.944		29.78	29.63	29.58	69	46						
20.	29.903	29.854		29.63	29.65	29.30	65	48						
21.	29.934	29.709		29.50	29.70	29.39	57	33						
22.	30.071	30.053		29.84	29.58	29.82	59	51						
23.	30.124	30.068		29.78	29.90	29.53	63	50						
24.	30.124	30.095		29.77	29.80	29.90	60	49						
25.	30.033	29.873		29.66	29.50	29.61	62	52						
26.	29.860	29.750		29.51	29.59	29.40	60	42						
27.	30.035	29.715		29.65	29.82	29.58	62	51						
28.	30.530	30.285		30.15	30.46	30.08	65	41						
29.	30.636	30.535		30.43	30.30	30.23	60	31						
30.	30.315	29.990		29.95	29.72	29.65	61	39						
31.	29.802	29.495		29.49	29.30	29.41	54	34						
Mean.	29.925	29.779		29.411	29.409	29.728	58.32	40.84					2.18	3.25
							45.19	46.75						2.85

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JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XXXV. THIRD SERIES.

LVIII. *On the Rotation of the Plane of Polarization of Heat by Magnetism.* By MM. F. DE LA PROVOSTAYE and P. DESAINS*.

SHORTLY after the brilliant discovery of Prof. Faraday of the rotation of the plane of polarization of light by magnetism, M. Wartmann announced † that he had tried the same experiment with radiant heat. He employed the heat of a lamp, which he partially polarized by making it pass through two piles of mica crossed at right angles. The electro-magnets and a cylinder of rock-salt were placed between these piles, and consequently very near the thermo-electric apparatus. The galvanometer, on the contrary, to be preserved from the action of the electro-magnets, was removed to a great distance; but the result was a considerable increase in the length of the circuit, and a diminution of sensitiveness.

Notwithstanding all these inconveniences, which he clearly pointed out, and which he was not able to overcome, M. Wartmann thought he observed that the needle of the galvanometer, after having attained a fixed deviation under the influence of the radiation not intercepted by the piles of mica, was again displaced and took a fixed position different from the first when the current was established, which seemed to indicate a rotation of the plane of polarization of heat.

At Paris, some persons having vainly attempted to reproduce these phænomena, we have considered that it would be useful to revert to these experiments, and to point out a method which permits of making them succeed with facility.

We have introduced into M. Wartmann's process three principal modifications:—1st, we employ solar heat; 2ndly, we take for polarizing apparatus two prisms of achromatic spar; 3rdly, and this appears to us indispensable, instead of placing the principal sections at 90° , we arrange them so that they make an angle of nearly 45° .

* From the *Annales de Chimie et de Physique*, October 1849.

† *Institut*, May 6th, 1846, No. 644.

The employment of spars and solar light permits of removing the electro-magnets to a great distance from the thermo-electric pile. With respect to the arrangement of the prisms, the law of Malus shows all the advantages which it presents. In fact, let us take for unity the deviation which the solar ray transmitted through the principal parallel sections would produce. The deviation, when the prisms form an angle of 45° , will be $\cos^2 45^\circ = \frac{1}{2}$. If the current is set in

action, and it produces a rotation of the plane of polarization equal to δ , the deviation will be, according to the direction of the current, $\cos^2(45^\circ - \delta)$ or $\cos^2(45^\circ + \delta)$, and we shall then have, for the difference of the effects observed when the current is made to pass in a contrary direction,

$$\cos^2(45^\circ - \delta) - \cos^2(45^\circ + \delta) = \sin 2\delta.$$

On placing the principal sections at 90° , the difference of the deviations would be only

$$\cos^2(90^\circ - \delta) - \cos^2 90^\circ = \sin^2 \delta,$$

or

$$\cos^2(90^\circ + \delta) - \cos^2 90^\circ = \sin^2 \delta.$$

Now $\sin^2 \delta$ is considerably less than $\sin 2\delta$. If, for example, we suppose $\delta = 8^\circ$, $\sin 2\delta$ is equal to more than fourteen times $\sin^2 \delta$.

The eye, it is true, appreciates readily the transition from darkness to light, but not so the difference in brightness of two luminous images. This is not the case with the thermoscopic apparatus. There is therefore, when heat is concerned, a great advantage in proceeding as above directed.

The following are the details of the experiment: the solar ray, reflected by a heliostat, traverses at first a doubly-refracting achromatic prism. The extraordinary bundle was intercepted: the ordinary bundle traverses the electro-magnet of M. Ruhmkorff's apparatus, and a flint-glass of 38 millimetres in thickness between the poles of the electro-magnet. It afterwards encounters, at about $3^m.50$, the second prism of spar, bifurcates again, and gives two images, one of which may be received on the thermo-electric pile placed at four metres from the electro-magnet. The galvanometer was still a little further removed from this disturbing force. It was ascertained, by direct and repeated experiments, that on establishing the current there were no phænomena of induction, and that the electro-magnets had no appreciable action on the magnetic needle which, under their influence, remained at zero in a state of perfect rest. In order to understand this, it must be borne in mind that the two opposite poles are very close together,

and that they act simultaneously upon a system already very distant and almost completely astatic. It might be feared that the electro-magnet, without action on the needle at zero, acted on the needle already displaced by the action of the calorific radiation. This would be possible in fact, if, in its first position, the needle had the same direction as the line which joins its centre to the electro-magnet, and if, when it deviates, it made a notable angle with that direction. In our experiments, precisely the inverse condition was realized; so that the component of the magnetic action diminished more and more during the movement of the needle, and became perceptibly null when it attained its greatest deviation. If therefore it had no action in the first case, such ought for a stronger reason to be the case in the second.

By means of a commutator the electric current could be made to pass, now in one direction, now in another, through the wires of the electric magnet. We shall designate the two currents by the abridged expressions *Current A*, *Current B*.

The following are the deviations observed:—

Experiments of September 22.

(A Muncke's battery of 50 elements with large surfaces, but already worn, was employed.)

First Series.

	Deviations.
Current A	21·0
Without current	19·0
Current A	21·4
Without current	18·6

Second Series.

(Acid was added.)

Without current	20·5
Without current	20·6
Current B	18·6
Without current	20·9
Current A	23·6
Current B	18·8
Current A	22·0
Current B	18·0
Without current	19·9

Third Series.

Current B	17·4
Current B	17·1
Current A	19·5
Without current	18·3

Experiments of September 29.

(A Bunsen's battery of 30 elements, well-cleaned and amalgamated, was employed.)

First Series.

	Deviations.
Without current . . .	12.0
Current A	14.9
Current B	8.6
Without current . . .	11.7
Current B	8.8
Without current . . .	11.8

Second Series.

Without current . . .	18.4
Current B	14.9
Current A	21.7

It is to be remarked, that here, if the principal sections of the prisms were perpendicular, the deviation, at first null, would scarcely attain one semi-division when one of the currents was made to act.

Lastly, to obviate every objection, a third series of experiments was made by taking away the prism of flint-glass, and observing the deviations produced by the solar ray, when, as before, the electric current was made to pass in the wires of the electro-magnet, now in one direction, now in another.

	Deviations.	} As should be the case, the deviations are equal, which proves that the electric current and the magnet change the deviations in acting on the flint-glass and not in acting on the needle of the galvanometer.
Current A ...	16.5	
Current B ...	16.8	
Current A ...	16.8	

The above experiments establish, we believe, in an irrefragable manner, the rotation of the plane of polarization of heat under the influence of magnetism.

LIX. *On a Granular Albite associated with Corundum, and on the Indianite of Bournon.* By B. SILLIMAN, Jun., M.D., Professor of Chemistry applied to the Arts in Yale College, and of Medical Chemistry and Toxicology in Louisville University, Kentucky*.

A SPECIMEN of a granular mineral was sent me by Mr. Gibbs of Andover, last year, with the remark that it was found in beds in Lancaster County, Pennsylvania, and

* From Silliman's Journal for November 1849.

was so hard as to resist all attempts to penetrate it by hardened steel, greatly impeding the operations of the miners in the chrome iron districts.

I also received other specimens of the same from Mr. Williams of Westchester, associated with corundum, which was found imbedded in it; and from this circumstance it has been mistaken by some mineralogists for Indianite, which species it resembles in hardness, gravity and in granular structure, but not at all in composition.

In its granular structure it so resembles dolomite, that no difference can be detected between them by the eye, while its hardness and great difficulty of fracture completely blind the inquirer as to its real character. Its characters are as follows:—

Massive, compact, granular, resembling white dolomite; tough; fracture even, but very difficult. Colour, white with shades of gray. Streak, white. Hardness, 7 to 7.25 (scratching quartz with facility). Gravity, 2.619.

Insoluble in acids. Before the blowpipe, infusible, and does not *colour the flame yellow*; with the fluxes yields evidence of silica, alumina and lime. By a quantitative fusion with carbonate of baryta, soda was detected.

The first specimen analysed was from Lancaster County, Pennsylvania, and showed no trace of corundum disseminated in it.

This analysis was made by Mr. G. J. Brush, and yielded on the quantity taken, 1.234 grm., as follows:—

			Oxygen.	
Silica	0.8225 = 66.653 p. c.	34.85 = 12	
Alumina	0.2565 20.786	10.70	3
Lime	0.0253 2.050	3.08	1
Magnesia	0.0071 0.519		
Soda	0.1155 9.360		
		1.2269 99.420		

It gives the constitution 4SiO^3 , $\text{Al}^2 \text{O}^3$, $\text{NaO} = \text{Al}^2 \text{O}^3$, $\text{SiO}^3 + \text{NaO}$, SiO^3 :—

4 atoms Silica	. .	2309.24	69.09 per cent.
1 atom Alumina	. .	642.33	19.22
1 atom Soda	. .	390.90	11.69
		3242.47	100.00

This is precisely the formula and constitution of an albite.

The second analysis was on a specimen from Unionville, Chester County, Pennsylvania, having identical characters,

but associated with corundum, which occurs implanted in it. This analysis was made by Mr. M. C. Weld. Quantity taken, 2.180 grms. Found—

				Oxygen.
Silica . . .	1.4575	= 66.857 p. c.	= 12	
Alumina . . .	0.4772	21.889		3
Lime . . .	0.0389	1.785	}	1
Magnesia . . .	0.0105	0.481		
Soda . . .	0.1914	8.779		
Water . . .	0.0105	0.481		
		2.1860	100.272	

This obviously yields the same formula as the last analysis.

The extreme hardness of this mineral is its most remarkable quality, and is not easily accounted for. It is probably connected with its association with corundum, for we find the quality equally developed in the Indianite (or anorthite), the Asiatic associate of the same species.

Analysis of Indianite.—I thought it of interest, in connexion with the foregoing analyses, to make a new analysis of Bournon's Indianite, which, as already remarked, is found to be the matrix of the corundum in India. Being possessed of an authentic specimen, I requested Mr. Brush to conduct the analysis, the results of which are now given. This mineral is granular, and of a pink colour, sometimes gray or blackish; very tough and hard. Hardness, 7 to 7.25. Gravity, 2.668. It gelatinized completely in cold hydrochloric acid. Before the blowpipe alone, infusible. The analysis gave on 1.594 grm.—

				Oxygen.
Silica . . .	0.6710	= 42.09 p. c.		21.869 = 4
Alumina and a trace of iron	} 0.6200	38.89		17.160 3
Lime . . .				
Soda . . .	0.2516	15.78	4.449	} 5.592 1
	0.0651	4.08	1.043	
		1.6077	100.84	



which is the formula for anorthite.

LX. *Supplementary Considerations to Mr. S. M. DRACH's Epicyclical Papers* (Phil. Mag. June to July 1849)*.

SINCE publishing my above-mentioned papers, I have unsuccessfully tried the general solution for more than two circles. However, knowing ϕ , the polycircloid

$$x = \Sigma (a_i \cos q_i \phi + b_i \cos p_i \phi = X_i = R_i \cos \Theta_i),$$

$$y = \Sigma (a_i \sin q_i \phi + b_i \sin p_i \phi = Y_i = R_i \sin \Theta_i),$$

may be regarded as the ultimate locus of a series of bicircloids, the centre of each being on the curve of its immediate predecessor, beginning with the centre of the deferent. With an odd value of i , one $b_i = 0$, and this last becomes a circle.

We have also

$$x = \Sigma a_i \cos(q_i \phi + t), \quad y = \Sigma a_i \sin(q_i \phi + t),$$

giving

$$r^2 = \Sigma a_i^2 + 2 \Sigma a_i a_j \cos q_i \phi - q_j \phi,$$

independent of the common constant t .

$$r = \Sigma a_i \cos(q_i \phi + t - \theta), \quad 0 = \Sigma a_i \sin(q_i \phi + t - \theta).$$

If the \cos angles in r^2 naturally arrange themselves as the powers of $\cos \psi = \cos p\phi - q\phi$, so that $\cos \psi$ is extractible by quadratic, cubic, or biquadratic equations, the solution is always analytically possible; *e. g.* the tricircloid

$$x = a \cos q\phi + b \cos p\phi + f \cos t\phi, \quad y = a \sin q\phi + b \sin p\phi + f \sin t\phi,$$

$$\therefore r^2 = a^2 + b^2 + f^2 + 2ab \cos p - q\phi + 2af \cos q - t\phi + 2bf \cos p - t\phi.$$

Let

$$\overline{p - q\phi} = \overline{q - t\phi} = \lambda\phi = \psi,$$

$$\therefore r^2 = a^2 + (b - f)^2 + 2ab + 2bf \cos \psi + 4bf \cos^2 \psi,$$

$$\therefore 4bf \cos \psi = -ab - af \pm \sqrt{4bf r^2 + (a^2 - 4bf)(b - f)^2}.$$

And then

$$2r^\lambda \cos \lambda\psi = \{a(c + s)^{q\phi} + \&c.\}^\lambda + \{a(c - s)^{q\phi} + \&c.\}^\lambda$$

$$= \Sigma \frac{\lambda(\lambda - 1) \dots (\lambda - i + 1)}{1 \cdot 2 \dots i} \times \frac{i(i - 1) \dots (i - j + 1)}{1 \cdot 2 \dots j}$$

$$\{c + s\}^{(\lambda - i)q\phi + (i - j)p\phi + jt\phi} = (q - 2j + i)\psi + (c - s)^{(q - 2j + i)\psi}$$

$$= 2 \cos (q - 2j + i)\psi.$$

Substituting the powers of $2 \cos \psi$ in $2 \cos (q - 2j + i)\psi$, we get the general equation of the curve.

* Communicated by the Author.

Ex. $p=3, q=2, t=1$, gives

$$8b \cos^3 \psi + 4a \cos^2 \psi + (b-3f)2 \cos \psi - 2a = 2x,$$

whence

$$4bf^3x = (3b+f)af(b^2-bf-r^2) - a^3b(b-f) \\ \pm (a^2b+fr^2-b^2f-bf^2) \sqrt{4bfr^2 + (a^2-4bf)(b-f)^2}$$

for the equation: if $b=f$,

$$2f^2x + 2ar^2 = \pm (a^2r - 2f^2r + r^3);$$

if $a=2f^2=2b^2$,

$$r^6 = (a^2x + 2ax^2)^2 = (x^2 + y^2)^3.$$

But in these quadratics we have a circumstance analogous to the discussion of the equations of the second order. For that $\cos \psi$ be real, the $\sqrt{\quad}$ must cover a *positive* quantity or zero; which, when b and f have different signs, shows that $4bf\{r^2 - (b-f)^2\}$ must be $< (ab-af)^2$; when $a^2=4bf$,

$$a \cos \psi = -b - f \pm r.$$

Similarly, $p=3q-2t$, $p=4q-3t$ would lead to a cubic or biquadratic equation. Even higher powers are thus resolvable, if the intermediate powers disappear through their coefficients vanishing, as

$$\cos^{12} \psi + 2A_1 \cos^6 \psi = A_2$$

gives

$$\cos \psi = \sqrt[6]{(-A_1 \pm \sqrt{A_1^2 + A_2})}.$$

The above trircircloidal expansion of $2r^\lambda \cos \lambda \theta$ exists *whatever* λ be assumed to be, and for \cos we may write \sin on each side of the equation.

The straight-lined, curved-cornered bicircloids, $\cos n\theta = \text{funct.}(r)$, are true *regular bicircloidal polygons of n sides*, corresponding to the angular ones of the simple circle (Euc. book iv.).

For the central bicircloids,

$$r = 2a \cos \frac{p-q}{p+q} \theta,$$

$$\frac{d^2(r^{-1})}{d\theta^2} \cdot r^{-2} + r^{-3} = r^{-3} + r^{-2} \left\{ \frac{d\theta d^2(r^{-1}) - d^2\theta d(r^{-1})}{d\phi^3} \right\} \frac{d\phi^3}{d\theta^3}$$

becomes

$$8(p-q)^2 a^2 r^{-5} + 4pqr^{-3},$$

\therefore when $q=0$ (excentric circle), as r^{-5} (Princ. I. vii. 1).

The length of these central bicircloids

$$= 2a \int d\theta \sqrt{1 - 4pq(p+q)^{-2} \sin^2(p+q)(p-q)^{-1} \theta},$$

an elliptic integral, except when $q=0$, $= 2a\theta$ (see fig. Euc. i. 1).

The loops of these curves are in general not analytically identical with the lemnoid resembling $\boxed{>O}$, where $q=2$, $p=3$,

$$8x^3a^{-3}-6xa^{-1}+2+4x^2b^{-2}=4r^2b^{-2}.$$

For in the other loops ($nn'=1$) $R^{2n'a^2}$ gives

$$\{2x^6b^4a^{-2}+\&c\}^{n'}=2X^{n'+1}-R^2(n'+1)(2X)^{n+1} \&c.;$$

so that x does not $=X$ generally.

The radius of curvature of the syphonoid

$$(x=a \cos q\phi, \quad y=\cos p\phi)$$

is

$$\{q^2(a^2-x^2)+p^2(b^2-y^2)\}^{\frac{3}{2}} \div \{p^2qy \sqrt{a^2-x^2}-q^2px \times \sqrt{b^2-y^2}\},$$

and its area

$$\int ydx=(p \sqrt{(a^2-x^2)(b^2-y^2)}+qxy) \div (q^2-p^2).$$

For the lemnoid

$$(x=a \cos q\phi, \quad y=y'=b \sin p\phi)$$

change $+q$ to $-q$ in these two expressions. Their arc-lengths are

$$\int d\phi \sqrt{a^2q^2 \sin^2 q\phi + b^2 \frac{\sin^2}{\cos^2} p\phi}$$

respectively.

The following table gives some results at critical points.

x.	y.	y'.	Rad. curvature.		Area.	
			Syphon.	Lemnoid.	Syphonoid.	Lemnoid.
a	b	0	0÷0	$p^2q^{-2}ba^{-1}$	$qab(q^3-p^2)^{-1}$	0
a	0	b	$p^2q^{-2}ba^{-1}$	0÷0	0	$qab(q^2-p^2)^{-1}$
0	b	0	0÷0	∞	$pab(q^2-p^2)^{-1}$	0
0	0	b	∞	0÷0	0	$pab(q^2-p^2)^{-1}$

Thus for the common parabola,

$$p=1, \quad q=2, \quad \frac{1}{4}ba^{-1}, \quad \frac{2}{3}ab, \quad \frac{1}{3}ab$$

are the respective quantities (the latter for half the area at the axis). Thus every lemnoid corresponds to a syphonoid.

The paragraphs in the Literary Gazette for March 28, 1846, were not penned by me, who had just made Mr. Perigal's acquaintance. Subsequent investigations have shown me that this Kinematic Parabola, being a finite portion of the common

conical one, has its x and y dependent on the periodic $\cos \mu$; which quantity enters explicitly and implicitly in the differential expression for the centripetal force to the focus, which is therefore not identical with the Newtonian law.

S. M. DRACH.

November 26, 1849.

LXI. *On Electricity and Steam*. By REUBEN PHILLIPS, Esq.*

35. **T**HIS paper is a continuation of a former one (Phil. Mag., vol. xxxiv. p. 502), wherein are described some magnetic actions produced by steam in the act of condensation. I regarded those motions as the effect of dynamic electricity, and then endeavoured to obtain the static effect, which I think I have done.

36. An electrometer was constructed in the following manner:—A straight slip of thin metal, about $\frac{1}{4}$ inch wide, was bent at some distance from one end, the direction of the motion in bending being in a plane perpendicular to the original surface of the plate and parallel to its length, and the bending was continued until the included angle was 90° ; the bend was opened a little, the end of a slip of gold-leaf inserted, and the whole rendered secure with a binding-screw. Previously, however, the plate at that part which was close to the end that had been doubled in, had been bent in the same direction as before, four times at right angles, so as to leave a little projection across the plate, over which the gold-leaf hung; the object being to keep the gold-leaf so far from the metal plate, that a rapid inspection sufficed to show that the former was free to move. The gold was the length of one side of the usual gold-leaf, and the metal plate extended parallel with it to its lower end. The doubled portion was now fitted into a slit in a cork, which was then inserted into a glass tube, the end of the bent portion projecting about $\frac{3}{4}$ inch beyond the external end of the cork. The other end of the glass tube also held a cork, through which a straight metal plate passed extending parallel to the gold-leaf. The tube was about $\frac{1}{2}$ inch diameter and $5\frac{1}{2}$ inches long, and the ends were covered with sealing-wax; however, care was always taken to connect the plate of the lower cork with the earth. The whole could be supported on a small wooden stand.

37. A singular appearance, but not connected with electricity, is presented by this electrometer when either of the metal plates is made to vibrate, as by a slight tap; for the

* Communicated by the Author.

gold-leaf is then powerfully attracted by, and drawn into contact with the vibrating plate. I suppose this observation is new, and very significant with regard to theories of attraction and repulsion, although I have not pursued it further.

38. As this electrometer was very sensitive, I more generally employed another, with two short gold leaves, and which was very far from being a sensitive instrument. This instrument was always employed except where I have mentioned that the single-leaf electrometer was used.

39. Throughout these experiments the steam was at 40 lbs. on the inch, and was supplied from the same little boiler and discharged horizontally as before (7.).

40. The Armstrong's condenser was removed, and the brass jet (9.) was united to the cock of the boiler by two short brass connecting pieces. This brass jet originally belonged to an oxyhydrogen blowpipe, and its shape was slender and conical, and the small hole at the end of the jet from which the steam escaped was $\frac{1}{4}$ inch long, the remaining portion of the bore was $\frac{1}{8}$ inch diameter. The boiler communicated with the electrometer by means of a wire. When the cock was opened, the boiler became negative.

41. In order to collect the electricity of the steam, I used a piece of wire-gauze, insulated and supported on a stand, and which by means of a wire could be made to impart its electricity to the electrometer; the extent of one side of the piece of wire-gauze was about 2.5 square inches. The electricity of the steam was thus found to be positive.

42. A straight glass tube about 3 feet long and $\frac{1}{4}$ inch diameter, was placed horizontally with the brass jet projecting into it to the extent of the $\frac{1}{3}$ th of an inch; the tube was also supported so that an annular space existed between the end of the jet and the tube. In order to ascertain the electrical state of the inside of this, or any other tube, a few inches of its length were covered externally with a piece of tin-foil and bound on with wire, the other end of which could be taken to the electrometer.

43. When the steam was turned on, the tube and boiler both became negative. A connexion existed between the tube and the boiler; for when the leaves of the electrometer were divergent, touching the boiler made them collapse. Sometimes the first action of the steam was to make the leaves diverge to a small extent, then to make them approximate a little, and then to cause them to open much more with a negative charge; but when the connexion between the tube and the electrometer was broken before the approximation took place, the instrument was found to be positively charged.

The wire-gauze being placed in the steam as it issued from the end of the glass tube showed that it was positive.

44. A glass tube, 3 feet 10 inches long and 1.1 inch diameter, was substituted for the former tube. With this tube all the foregoing effects were observed, but they were more powerful.

45. These experiments are only exhibitions of frictional electricity; and the different states of the tube are principally produced by the tube taking its charge, either from the brass jet or from the excited steam that issued from it.

46. The Armstrong's condenser was now interposed, with water in it, between the boiler and the brass jet; and the smaller tube (42.) had the end which came to the brass jet fitted with a good cork, through which the brass jet passed and projected into the tube much as before; the joint was then made tight with caoutchouc and thread. To ascertain the state of the steam, I employed a piece of wire gauze, $6\frac{1}{2}$ inches by 4 inches, attached by means of stout wire to an insulating handle, by which the gauze could be held in the steam and then taken to an electrometer. This wire-gauze collector was generally used in the following experiments.

47. The steam being turned on, the tube and boiler became positive. When the wire-gauze was held at about an inch or two from the end of the tube, it took from the steam a negative charge; and at the distance of something more than a foot, the gauze also acquired a negative charge which affected the single-leaf electrometer; and at intermediate positions the charge was still negative.

48. When the condenser was removed, and the jet was screwed into the cock of the boiler, everything else being as before (46.), no decided electrical effects were observed; but when the glass tube was also removed, then, having connected the boiler with the single-leaf electrometer and cautiously turning the cock, it was discovered that at a particular pressure the boiler became positive, and at much higher or lower pressures negative.

49. Now I regard these electrical effects as frictional, and possibly similar to those alternations in the electrical state of the boiler observed by Mr. Armstrong. (*Phil. Mag.* vol. xxiii. p. 202.)

50. While the steam was escaping into the air from the brass jet as above, I made some attempts to ascertain whether the steam was positive or negative; the electricity was very feeble, on which account I did not care much about it, but the little that I obtained was decidedly positive. This may have arisen, although it is improbable, from the boiler becoming

negative, but it is more likely that the steam was rendered feebly positive from a cause which I now proceed to mention.

51. The condenser and jet were arranged as in the experiment (46.), and the smaller glass tube instead of being fastened on with cork and India rubber stood before the jet as formerly (42.). The steam being now turned on, the boiler and tube became positive. When the wire-gauze was about 1 inch from that end of the glass tube from which the steam was escaping, it took a negative charge; but at the distance of about a foot, the larger wire-gauze collector being employed, the steam communicated a powerful positive charge to the single-leaf electrometer. The electricity of the boiler and tube, and the positive electricity of the steam, were much stronger than the positive and negative electricities of the preceding experiment with a tight joint between the tube and jet. The negative charge was most probably given to the gauze by the friction occasioned by the violent rush of steam and air against it.

52. The larger glass tube (44.) was put in the place of the foregoing and carefully adjusted like the former tube, so that the central line of the path of the steam should lie as nearly as possible in the axis of the tube. The boiler and tube were positive. A connexion existed between the tube and boiler, for when the steam was passing and the leaves of the electrometer were greatly divergent, touching the boiler made them collapse to a comparatively small amount. When the gauze collector was held at about an inch from the end of the tube, a positive charge was obtained, and at a distance of 9 inches a powerful positive charge was still obtained; further experiments in this direction were prevented by a wall.

53. By means of India rubber, the end of the glass tube at the brass jet was closed, which caused the electrical effects to diminish to such an extent that the single-leaf electrometer indicated but doubtful traces from either the tube or steam.

54. A short glass tube, 14 inches long and of the same bore as the preceding, was substituted for it. The tube was always positive, and the boiler, except in one instance, neutral or positive. The larger collector being held, from that end of the tube from which the steam was escaping, at distances varying from 1 inch to 2 feet 6 inches, gave always a powerful positive charge.

55. The glass tube was now removed and the larger collector held in the steam at about 9 inches from the end of the brass jet; the boiler, collector and electrometer being electrically united. An abundance of positive electricity was obtained, which would doubtless have been obtained at a much greater distance.

56. The piece of a gun-barrel (20.) was now placed with proper precautions before the jet, and by means of wire connected with the boiler. The boiler was positive, and the steam powerfully positive even at a distance from the jet of 4 feet 3 inches, the current of air at that distance being a mere breeze; and I doubt not that had it not been for the wall, I could easily have obtained plus electricity at double the distance. From this it clearly follows that the plus electricity of the steam cannot be generated by friction against the gauze. Neither does friction in the tube generate it, for the effect was obtained without a tube (55.); nor can it be generated by friction in the brass jet, for this frictional electricity was found to leave the boiler negative, which negative electricity was not nearly equivalent to the positive of the steam (55.); to which I may add the similar experiments with the tubes, the tubes being evidently collectors only and not generators of the electricity of the steam.

57. It would seem as though positive electricity only, without negative, was generated in these experiments; however, it is not so.

58. The single-leaf electrometer was now placed in the vial-holder of a microscope, through which the motions of the gold-leaf were observed by a power of about 200 diameters, a micrometer eye-piece being employed. Also a thin piece of metal, 2 inches by 6 inches, was fixed on an insulated support, the greater length of the plate being about perpendicular to the horizon. This metal plate was now placed at a little distance from the end of the former gun-barrel arrangement (56.), and sufficiently distant from the path of the steam to be out of the way of the straggling drops of water; this screen was now connected with the electrometer, and the boiler carefully connected with the earth. When the steam was turned on, the electrometer received a negative charge. The extent of the motion of the leaf was very variable, depending, I think, on the amount of water discharged with the steam; on the average, I reckoned it about five micrometrical divisions. A brass tube, 6 inches long and $\frac{17}{20}$ inch diameter, was now jammed on the end of the gun-barrel, to increase its length, and the inductive plate was placed at the end of it, as before at the end of the gun-barrel. The steam now being turned on, the leaf moved over between twenty and thirty divisions.

59. It was easy to become convinced that these motions of the leaf were really produced by induction, by turning on the steam and allowing the leaf to obtain its deviation and then quickly shutting off the steam, when the leaf would promptly fall back. Also with the brass tube on, I managed to charge the instrument positively by turning on the steam, then open-

ing a communication for a moment between the earth and the plate, and then shutting off the steam.

60. When the brass tube was on the end of the gun-barrel, the leaf seldom returned to its starting-point when the current of steam was stopped, and the instrument appeared to have acquired a permanent charge, which was negative, and appeared to go on increasing with the time that the steam was allowed to escape. It was found, too, that as the inductive plate was removed in a direction perpendicular to the path of the steam, the first-mentioned effect of induction rapidly diminished, but that the power which appeared to give the permanent charge scarcely underwent any alteration, even when the distance from the path of the steam was increased to 9 inches, the greatest distance the apparatus would allow. I have but little doubt that this apparently permanent charge was principally produced by the inductive action of the steam-cloud.

61. From the foregoing it is seen that the jet of steam imparts positive electricity to any substance that it touches, while at the same time the steam, as a whole, may be more or less negative. These facts will very well comport with the idea that the particles of water are positive and the gaseous matter negative; hence when the current passes through a tube or touches a piece of wire-gauze, the particles of water become more or less separated from the air and vapour, leaving them negative. That the negative electricity of the gaseous matter is not immediately transferred to the particles of water, is, I suppose, due to the non-conducting power of gases.

62. When these electric effects are being produced by the jet of steam, a peculiar rough sound is heard, which is dependent on the discharge of water with the steam; for when the jet was screwed into the cock of the boiler, I never heard any other sound than a smooth sibilation; and when water was discharged with the steam, if its quantity was not quite sufficient, the smooth hiss only was obtained. At such times, the steam, as it issued from the jet, on being examined, was found to be, for the distance of $\frac{1}{8}$ th of an inch from the end of the jet, transparent, with the exception of a thread-like appearance of cloud extending through the clear steam parallel to its path. Presently, from a lowering of the pressure, or other cause, this little cloud expanded all round the jet, and then looked like a frustum of a little right cone of cloud stuck on the end of the brass jet, the roar being at the same time perfectly developed. These effects were obtained without any tube before the jet.

63. With the arrangement described (51.) very instructive results were occasionally obtained; the tube was connected

with the electrometer. Sometimes, when the steam was turned on, the roar was absent, and then the electrometer was with difficulty acted on; then suddenly the roar was set up, and the electrometer was promptly and powerfully affected.

64. It appears to me that when water is discharged with the steam and a smooth sibilant sound only obtained, that the water passes off as a little stream from one part of the orifice, but that with the roar the water escapes about equally from each part of the hole. In the latter case it is doubtless much more finely comminuted.

65. When a jet of steam and water thus escapes into the air, the particles of water, from their size and consequent weight, will soon move faster than the atoms of water which were discharged in the gaseous state. From this follows the question, Are the particles of water charged by rubbing against the gaseous matter? To this it must be answered, that this electricity was not generated in the former experiments (46, 53.), where water, gaseous matter and friction were present. Also, no experiment is known in which gaseous matter is certainly charged electrically by friction. (See Dr. Faraday's paper on Steam Electricity, *Experimental Researches in Electricity*, vol. ii. p. 106.)

66. A jet of steam being discharged into the air becomes magnetic (11.), which I attribute to currents of electricity passing from the hotter to the colder particles (33.), in this case steam and air; the steam, or water which was discharged as steam, being positive, and the air negative. When particles of water are projected through the steam-cloud, I suppose they collect together the minute positive particles, thus becoming themselves positively electrified, leaving the gaseous matter negative. I have yet made no direct experiments to ascertain the minimum pressure at which these effects are produced, but I am certain they may be obtained at a much lower pressure than 40 lbs. on the inch.

67. Since these electrical effects are produced by the transmission of drops of water through a steam-cloud in the act of formation, it appears to me that these electrical developments may occur in the atmosphere. In my experiments, the drops of water were so fine that they were separated with difficulty from the air, but I see no reason for supposing that the electrical action would be essentially different if the drops were so much larger as to separate themselves by their weight. Lightning will thus result from a rapid condensation, the descending rain and mist being positive, leaving the upper regions negative.

68. I would have this explanation of the cause of lightning

to depend, at present, as little as possible on the mechanical violence of the drops of water; because some experiments, not yet fit for publication, seem to show that when steam and air at the same temperature are discharged into the atmosphere, the amount of water simultaneously discharged may be greatly reduced, perhaps to nothing.

69. The negative state of the air, which one would expect to find particularly strong just after a thunder-storm, may slowly discharge itself on the earth by conduction, or on the clouds, or rain, and thus to the earth by convection; nevertheless the direct tendency of lightning, according to this theory, is to render the upper regions negative; and the facilities for the return of this negative charge to the earth will decrease in some proportion as the altitude increases. The continual thunder-storms of the hotter climates may therefore gradually accumulate a powerful negative charge in the heavens. Suppose now, when the air is thus highly charged, that a column of mist sufficiently high and dense to act the part of an electric conductor to the positive earth and negative heavens should be interposed between them, the electricity would then pass as a series of disruptive discharges, and would, I suppose, be the aurora.

7 Prospect Place, Ball's Pond Road,
3rd December 1849.

LXII. *Geometry and Geometers. Collected by T. S. DAVIES, Esq., F.R.S. and F.S.A.**

No. IV.

THE printing of the three letters of John Bernoulli to Cramer, which I had designed to give as the next of this series of "gossipings," must be deferred for awhile; as, from particular considerations, I am led to think it desirable to confine myself, for the present at least, to matters of more purely English interest.

In 1747 Mr. Thomas Simpson published the first edition of his *Elements of Geometry*; and in 1756 Dr. Simson the first edition of his *Euclid*. In this latter work are two notes upon objections which had been raised in the former, to some of Euclid's processes. In 1760 Mr. Simpson published the second edition of his *Elements*, in which he defends himself with great earnestness on those topics; and Dr. Hutton (in his *Life of Simpson*, prefixed to the *Select Exercises*, ed. of

* Communicated by the Author.

1792) states that these criticisms of Dr. Simson's gave him "some trouble and vexation,"—though on what ground, except his own sensitive nature under prostrated health and its consequent depression of spirits, it would be hard to tell. The man who could maintain his temper under the offensive ribaldry of the notorious Robert Heath, could not on any other supposition than this, feel wounded *very deeply* by the plain, though, it must be confessed, somewhat contemptuous language of Simson, in those notes. Dr. Hutton further says, that Dr. Simson again replied in the notes to his next edition of Euclid (Life of Simpson, p. xi.). Also Dr. Trail (Life of Simson, p. 31), remarks that "some animadversions were made on this edition (1756) chiefly by those whose works had been criticised in the Doctor's notes; and to some of these, in a second edition, replies and explanations were made." These, of course, Simpson never read, as the edition referred to was not actually published till 1762, whilst Simpson died the year before, and had been some months prior to this in a state that precluded reading and study of every kind. Nor is there in the notes at the end of the second edition of his *Geometry* anything to induce a belief that he wrote them under any irritable feelings; and we have his distinct averment, at p. 263 (I quote from the fourth edition, which *seems* to be paged precisely as the previous two), that he was ignorant of the existence of Dr. Simson's Euclid till the middle of November 1759, and his own work is dated March 3 of the following year, when he was in a most enfeebled condition. This was indeed his last connexion with the press, if we except his preparation of the *Ladies' Diary* for the following year; in which, however, there is reason to believe that he was greatly assisted by his friend and successor, Edward Rolin-son.

I have collated with some care the one hundred and five notes given by Dr. Simson in the first and second editions of his Euclid. There are but few variations in the greater part of them; and even these merely verbal, or of the most casual kind. As regards our present purpose, it is enough to state that in the 8vo edition Simpson is referred to four times, and in the 4to only twice; and that these two notes are alike in both those editions. They are upon *i.* 22, 24 and 25, and *xi.* 1; the second and fourth of these *first* appearing in the 8vo edition.

Dr. Simson, however, wrote a more expanded series of criticisms on Mr. Simpson's notes, a copy of which (in *Nourse's* hand) is amongst the papers I formerly described. An exact transcript of the original it evidently is; and a memorandum

at the head identifies its authorship and purpose. The paper itself has much the appearance of being intended for press, though there is not the least reason to think it ever was printed. On this account I give it entire. Probably it was cancelled on account of Simpson's intimacy with Nourse.

"Remarks upon some of Mr. Thomas Simpson's notes at the end of the second edition of his *Elements of Geometry*.

"*Art.* 1. In page 257 and the next there is a long note, one design of which is to show the impropriety of conceiving one figure to be transferred from one place to another when the thing in hand can be done another way; he says Euclid has never recourse to this in other cases; but Euclid not only uses it in the 4 Prop. 1 and in the 24 Prop. 3. but also in the 4 Prop. 6. where he might instead of conceiving one of the triangles contiguous to the other with its base in the same straight line with the base of the other, have constructed a triangle in that position, having its sides equal, each to each, to the sides of the other; and in like manner the corollary of 1 Prop. 6 might have been shewn; but it would have been quite needless in either case. Mr. Simpson himself uses this method in the axiom before his 7th Book, where without the least necessity he conceives the figure PQR to be formed equal and similar to the bases ABC, DEF and the prisms upon these last bases to be placed upon the base PQR; whereas he needed only have conceived the prism on the base ABC to be applied to the prism on the base DEF so as their bases may coincide; but he has been afraid that they could not stand both together upon one base, and therefore he bids place them successively upon the base PQR."

The reader will remark the quaint humour of the close of this article: but long experience convinces me that very few pupils are able to abstract all notions of *impenetrability* when considering the geometry of *solids*. The term "solid" is indeed an unfortunate one to have chosen, by which to designate a figure of three dimensions; and it is difficult to remove from the young mind some vague notion of the impossibility (or absurdity of directing it) of applying one solid to another so that both shall be in the same place at the same time. The method of Simson's Euclid is unquestionably legitimate: but something must be conceded in the outset of a new course of study to the youthful incapacity for *abstraction*. The conception of both solids being successively fitted into the same matrix, or in the form that Simpson gives it, is less likely to violate previous notions, than the more abstract and certainly more philosophical process. Neither is it inaccurate under any aspect as a method of reasoning; and the more desirable view will easily present itself to the mind of the careful student at a stage of reading not very remote.

But is the supraposition of a *plane* figure upon another

plane figure materially different from this? For one line to be *on* another is surely not "coincidence" in the strict sense of the word; and certainly there is little reason to consider an argument founded upon two lines so situated, as a legitimate proof. Yet how few who have read Euclid have any other idea of supraposition! Euclid does not use the term; and no commentator or teacher who does use it, would pass it by without showing in what sense it is used. This is easily done by recalling the definitions of the point, the line and the plane, to the student's mind. The difficulty of perfect abstraction of some material properties is of the same nature here as in the case of "solids;" but it is much simpler, and more easily removed from the young mind.

As regards the *legitimacy* of the processes of transferring figures, so much has been said on both sides that it might be difficult to propound a view which somebody or other has not taken before. One thing, however, is clear; that no geometer has been successful in attempting to build up a legitimate system without the aid of this conception—either openly or in disguise. We are forced back, then, upon the *necessity* of using it; and perhaps our notions of legitimacy may be of too exalted a character to be ever realised. Are we sure, indeed, that our conceptions of form and the other properties of figure are not so obtained by us initially as to necessarily involve the consideration of transfer? Many considerations are involved in this question which lie beyond the ordinary range of geometrical discussion. It was evidently Euclid's desideratum to evade it; and yet he was unable to do so in *i.* 4, *i.* 7, and *iii.* 24: nor has any one else succeeded in doing so with perfect strictness.

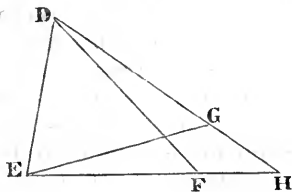
That this method of transferring figures was amongst the *very earliest modes of demonstration*, there can be no room to doubt; and that it was much more used (even in the school of Plato) before the time of Euclid, appears to me extremely probable. Little, perhaps nothing, is likely to be found of a documentary character either to bear out this view or to sweep it away. It is a *natural* mode of proceeding, and it is in a few cases even yet an *inevitable* one. The refinements introduced into geometrical reasoning by Euclid, or perhaps partially so before his time, induced the attempt to eliminate all idea of *motion*, as one of the properties of matter. It is on the same ground that corresponding modern refinements have been attempted, in which the principle should be completely carried out. That the conditions which determined the equality of two triangles were not determined *originally* without the adoption of the principle of transfer is almost certain; and as to many pro-

perties of the circle in the third book, the two circles being left (both in the enunciation and demonstration), can only be accounted for on the hypothesis that the proofs we now have were modified from proofs by transfer. This (as well as the fact that most of them would be more neatly exhibited in reference to a single circle than to two) would seem to point out the change as an *immediately recent one*—most probably Euclid's own change. I cannot, however, further enlarge here upon this question.

“2. P. 261. The reasoning here is such that it is difficult to know what his meaning is. ‘he says the question here, is, whether a triangle, under certain specified conditions, can, or cannot be formed? and therefor[e] to conclude any thing from the properties of Triangles would be ridiculous, and nothing less than begging the question.’ does he mean that because the triangle is to be made, and not yet formed that therefore it is not allowable to conclude anything from the properties of triangles which have been already demonstrated? Were this true, he would, indeed, be in the right to affirm as he does that the problem has a limitation, yet it would be absurd to urge it in this case: he makes use of his own limitation in this Problem, and why may not Euclid make use of his which is easier to be understood, and the consequence from it that the circles will meet sufficiently plain. But really this deserves no serious reply.”

That the conclusion of the intersection of the circles can be obtained from Euclid's limitation (*viz.* that any two sides of a triangle are greater than the third), is true enough: but that it is actually made to follow by any reason beyond “inspection” no one surely will say. A process tantamount to Simpson's *is essential*; and whilst many may think that the objection does “deserve a serious reply,” no one will, I think, consider what is given above as any reply at all.

“3. In the same note he observes that the point F ought to have been shown to fall below the line EG (or rather, because the point G is found by construction, that the point G falls above EF.) this probably Euclid omitted, as it is easy to see that DF being greater than DE the angle DFE is less than a right angle, and producing EF to H that therefore DFH is an obtuse angle, and consequently any straight line DH drawn to EF produced must be greater than DE, and that DG which is equal to it must fall above EH.”



That Simpson's objection was a valid one is now unques-

tioned: whether the present completion of it, or that given in the note upon *i. 24* be satisfactory or not, will give rise to different opinions. It will be necessary to *prove* what Dr. Simson assumes in the opening of the above reply, that DFE is less than a right angle; and as to that in the note in Euclid, there is an assumption not at all warranted by anything prior to *i. 24*:—except, indeed, the warranty of his own precedent in *i. 13*.

Simpson administers a very proper and dignified rebuke to the Doctor in the same note, but which is not noticed either in this MS. or in print.

“Professor Simson (at p. 359 of his Euclid, 4to edit. 1756) has been a little severe upon me, on this head, for attempting to supply, what I thought a small defect in *Euclid*. ‘Who is so dull (says he) tho’ only beginning to learn the Elements, as not to perceive that the circle described from the centre F &c.’ It is not without a real concern that I here see this able Geometer drop his own character so far as to express himself in a manner so very *ungeometrical*. If the thing is indeed so easy to be perceived, it must be so either as an object of the senses, that is, in plain terms, by inspection; or else it must be in consequence of geometrical reasonings antecedent to the thing itself. Now I am clear that he would not be thought to mean the former; and as to the latter nothing had been given from which the evidence of the inference could be so clearly seen: For though, &c.”

It may be added, that a particular object induced me a little time ago to note the *assumptions that are tacitly* made in the first book of Euclid. Whoever does the same will not be a little surprised at their number.

There is no doubt that Simson removed a great number of small blemishes from Euclid’s Elements; but, it is equally certain, that he has still left a great many more than he has removed. He found some of them too firmly rooted into the *system* to be able to eradicate them without venturing upon far greater changes than he has done—and he showed no “lack of courage” in that way, either. The present is one of them; for the proof ought to form part of the text; whereas it is treated in a note as a thing “easy to see.” The same assumption in a still more objectionable connexion occurs in the construction of *i. 13*. For there it is assumed that the circle whose centre is C, and which passes through a point D on the other side of the line AB, will cut that line in *two* points, F and G; as otherwise the construction following could not be performed. It is also to be presumed that the circle can *only* cut the line in those two points; as if it cut in more than

two, say n , then there could be $\frac{n(n-1)}{2}$ perpendiculars drawn from C to AB. The line FG must then fall *wholly within* the circle; whereas when we come to the third book we are required to prove this very property; and more strangely still, to draw a straight line *within* the circle in the antecedent proposition. The difficulty is inherent in the method pursued. *It compels the eye to supply the place of reason. We merely see that it is:—not know why it must be so.* It is not by such phrases as “who is so dull as not to see?” or “it is easy to see,” that this blemish can be removed.

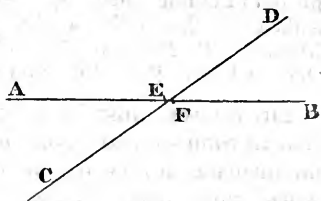
I believe that no one who has been much concerned in mathematical education will complain of students “only beginning the Elements” being very short-sighted upon such points as these. They find it “easy to see” that many things are so and so which the tutor would prefer their *proving*. In these cases, it is not the fact that is in question, but its place in the logical system. Geometry by inspection is always more acceptable to the majority of young students than geometry by demonstration. Mr. Byrne must have known this, when he carried the system to perfection in his Euclid in Colours. Did geometry, however, stop at *visible properties*, it would be jejune enough. I have often found, too, that the notion of the ultimate objects of geometrical research being principally graphic, or in some other way merely practical, has, whilst it has fostered the propensity to proof by inspection, generally led to much confusion in the student’s mind. Let it *always* be presented to his mind as a rational science—*never* as a practical art, except, indeed, incidentally.

“4. In the same place he blames the bidding ‘draw a straight line within a circle, without specifying that it must terminate in the circumference’ not knowing that this way of speaking is constantly used by Euclid and other Geometers; as not only in this 1st. of the 3^d but in the 4, 14, 15, 28 of the same book, and though the 2 Prop. of the 3^d is to prove that a straight line joining *any* two points of the circumference is within the circle, yet surely there is no need of demonstrating that a straight line may be drawn somewhere within the circle; the 2^d Prop. being to shew that though the points be taken never so near to one another, that the straight line joining them will neither fall upon nor without the circumference.”

Simpson had only noticed this as an instance of vague expression. Is it not such? Is it not more—even much more?

“5. In the same note he answers what is said in page 415. of the 4^{to} English Edition, that it ought to be demonstrated not assumed that a straight line cannot meet a straight line in more than one point, and says it cannot be demonstrated, and that the Editor refers

it to the 14th axiom of Book 1. (he means in Barrow's Euclid, for it is the 10th in the Greek). but all that in this case follows from that axiom is that if two straight lines AB, CD could meet with each other in two points E, F, the parts of them between E F must coincide, and so A B, C D would have the segment common to both; but this does not prove that they cannot meet in two points, from which their not having a common segment is deduced in the Greek Edition. but because they cannot have a common segment, as is shewn in Cor. of 11 Prop. 1. 4^{to} Edition it follows they cannot meet in two points."



As regards a logical system, Dr. Simson is right in this view; but in all that relates to the line and plane, especially the first steps of deduction, there exists in most minds some degree of confusion, arising from the vagueness of the definitions of those objects of our contemplation. In discussing Euclid's method, it appears to be almost always forgotten that his *metaphysical creed* was Platonism; and questions arising out of such antecedent creeds always must present themselves in the initial steps of a science.

It cannot be denied that Simpson might have retorted, had he seen this, "who is *so dull* as not to perceive that two straight lines cannot meet in two points?" It would be an assumption far less removed from our first conceptions than some that Euclid makes and Simson justifies.

"6. P. 265. The observation at the end of the note which begins in p. 264, viz: that in the Corollary (of Prop 11 Book 1.) the lines A B, B D, B C are supposed to be all in the same plane, which cannot be assumed in 1 Prop. 11. is very just. Soon after the 4^{to} Edition was published I observed this error and corrected it in the way Mr. Simpson has mentioned in this note. he is mistaken in thinking the 10th axiom he mentions here, to be Euclid's, the Axiom he means is That two straight lines cannot have a common segment, which is none of Euclid's, but is the 10th. in Dr. Barrow's Edition, who took it from Herigon's Cursus vol. 1. and to supply this, is the design of Corollary 11 Prop. 1.

"7. Page 267. and the following contain several objections against Euclid's doctrine of Proportionals, most of which with many others, have been long ago distinctly and fully answered by the learned Dr. Barrow in the 7th and 8th of his Mathematical Lectures. Some of which relate to the notes on the 4^{to} Euclid are as follows.

"8. At the bottom of P. 270 he says that 'Euclid or Eudoxus does never (that I know of) refer to any definition till he has proved either by actual construction, or by some Demonstration previous to that in hand, that such definition involves no absurdity &c.' the

contrary of this is manifest, for instance in the 4th Prop. 5 he refers to (or makes use of) 5 Def. 5 without ever having mentioned it, or proved anything about it, after he had given it. Dr. Barrow has sufficiently shewn the weakness of such objections; see his Lect. Mathem: P. 299 near the end, and the sentence which begins in the last line of Page 303. *nam si &c.*"

Dr. Simson must have been "hard driven" for an objection to Simpson, to have quoted this case: inasmuch as it so far furnishes a new argument against the *perfection* of Euclid's work; and thereby affixes a stigma to the "4^{to} Edition," which is as yet unremoved from any subsequent one. That the canon enforced by Simpson is a safe, a sound, and an essential one, who will deny? Nay, more:—not only should the *consistency* of the definition be shown, but its *completeness*, and its *freedom from redundancy*, likewise. There is perhaps no instance in Euclid of incompleteness of defining conditions: but there are several of redundancy—such as the square, and similar rectilinear figures, so often quoted by editors and commentators. The mischief done in such cases as these is rather done to the logic than to the geometrical facts: but then, on the other hand, these superfluous conditions might be contradictory to the essential ones, just as easily as consistent with them, or consequential from them.

Instead of collecting all the definitions at the beginning of a book or treatise, many judicious writers prefer to give them *pari passu*, as the occasion for them, as well as their justification, shall present itself. Probably the fifth definition of the fifth book and those connected essentially with it, may present a good argument in favour of such a mode. In more elaborate researches, and especially in the theories of modern geometry, this becomes almost indispensable; as for instance in the doctrine of "radical axes," "similitude," "poles and polars," "anharmonic ratios" and "involution." Many points, lines, circles, ratios, and relations of different kinds here *demand* specific names, which till their *necessary existence* has been proved, it would be a manifest absurdity to confer upon them. Under all circumstances, at any rate, whatever arrangement may be given to the definitions in a printed book, it is essential to observe the canon enforced by Simpson, as above quoted, in giving the composition and logical place of every definition.

It would require more space than I have at my disposal to enter upon the question of Euclid's fifth definition of his fifth book; and as I cannot treat it adequately, it must be passed over altogether, till some future time. For the same reason, I shall here offer no remark upon the portions of this paper of Simson's, which bear upon the fifth book.

Yet whilst I avoid such extended discussions as I should be entangled in by this subject, it is certainly due to Thomas Simpson to state, that all geometers have not entertained, and do not now entertain, that high estimate of Dr. Barrow's treatment of ratio that Dr. Simson did, and which he here so triumphantly quotes. It is satisfactory to be able to state my own view in the felicitous language of a dispassionate judge, and of one whose philosophical acumen in such matters needs no praise of mine, Professor Powell. In a paper read before the Ashmolean Society of Oxford in 1836, he thus expresses himself:—"Dr. Barrow, in his celebrated Mathematical Lectures for 1666, has treated the whole subject of ratios and proportion in the most copious and elaborate manner, but, as appears to me, with more learning than perspicuity; he is extensively occupied in examining and refuting such objections as those just adverted to; and in doing so seems more explicit and satisfactory, than in any attempts to elucidate directly the doctrine itself on real philosophical principles. In point of fact, in the midst of his very extensive dissertations, it is far from an easy matter to discover what is his own idea of the nature of ratios; and when it is developed, it is by no means clear wherein it substantially differs from the views of some of his opponents" (p. 14). I not only fully concur in this view, but I believe that much of the praise bestowed upon Dr. Barrow's Lectures on this subject, has been bestowed for fashion's sake, and not from those writers having read and unravelled that complicated series of discussions.

"9. In P. 379 of the 4^{to} English Edition it is said 'That in order to prove what is affirmed in the Demonstration of 10. Prop. 5 in the Greek Edition viz: that A cannot have a less ratio to C than B has to it, it ought to have been shewn that if the ratio of A to C be greater than that of B to C. and taking any equimultiples of A and B, and any multiple of C., the multiple of A is ever greater than that of C whenever the multiple of B is greater than that of C; but this is not done in the 10 Prop. but would easily follow from it and cannot without it be easily demonstrated. The author of the notes on the 8^{vo} [4^{to}? the 8vo was not published till after Simpson's death] Edition says, that this point ought to have been cleared up by propositions antecedent thereto and independent thereupon (I suppose he means upon the 10th. Proposition, else I do not understand him) and adds that the 8th Prop. seems the proper place for doing it. then he gives an account of what is proved in Prop. 8. and next gives a demonstration that the ratio of A to C cannot be less than the ratio of B to C. but in the very first words of it, he supposes that A is greater than B, which is the thing that is proved in the 10th Prop. and therefore he has done it by the help of the 10th, tho, a little before, he had said it should be done by propositions independent upon it; his demonstration is exactly the same with that at the

end the notes on the 10. Prop. Page 380 of the English 4^{to} Edition which he says ought to have preceded the 10th. it can, it is true, be done without the 10th but not so easily.

“ 10. In P. 271. at the bottom, he says ‘ the same objection occurs again in the 13th Prop. where it remains in its full force, for ‘ though it be allowed that there are some equimultiples of C and E ‘ and some of D and F such, that the multiple of C is greater than ‘ the multiple of D but the multiple of E not greater than the multiple ‘ of F; yet it is not demonstrated nor in any sort shewn, that other ‘ multiples of these quantities cannot be taken such that the very contrary shall happen.’ No indeed, for the very contrary may often happen; but this has not the least force against the demonstration Euclid gives of the 13th Prop. for since it is allowed, that such equimultiples can be taken as just now mentioned, the Demonstration of the 13th remains firm and legitimate, but indeed what he says here is so far from having the least appearance of an objection, that the mildest thing [that] can be said of it is, that the author has been in so great a hurry as not to have taken time to consider what he was writing.

“ 11. In P. 272. at the foot, he mentions the principle whereby the difficulty he had before spoken of might be obviated, viz : ‘ that if a ‘ magnitude of any kind be given, or propounded, there may (or can) ‘ be another magnitude of the same kind which shall have to it any ‘ ratio assigned,’ or which is the same thing, that unto any three magnitudes two of which are of the same kind, there can be found a fourth proportional. This he says I will by no means admit of (tho Euclid himself in 2. Prop. 12. has used it) and in P. 70 of his Elements at the foot he had said that ‘ this kind of argumentation ‘ is authorised and adopted by Euclid himself in his twelfth book’ as to which is to be observed that Euclid in the 12 Prop. 6 has shewn that a fourth proportional can be found to any three straight lines, from which what he assumes in the 2 Prop. 12 can be legitimately deduced, as is shewn in the note at the foot of the page in the 4^{to} Edition [and in all subsequent ones the same note is retained] at that proposition. but his using here will not infer that he would have used it before he had shewn the 12. Prop. 6.

“ 12. Near the end of P. 274 the author is for ‘ entirely rejecting ‘ the 10th and 13th Propp. of Book 5th and everything else founded on ‘ the Definition of a greater and less ratio, as being of no other use in ‘ the Elements than to open the way to these important theorems on ‘ the alternation and equality of ratios’ (by the last he means the 22 Prop. 5.) ‘ which may be better demonstrated without them, from the ‘ Definition of equal ratios alone, &c.’ but tho the 14th and 20th Propp. 5 on which the 16. 22. depend, can be shewn without the help of the 10th and 13th yet since these two last are frequently used both by antient and modern geometers, they ought to have a place in the Elements; and since they are there, it was proper to demonstrate the 14th and 20th by them as it is done without any construction by taking equimultiples.

“ 13. As to what is said concerning a nature or idea (‘ see P. 273’

in margin) antecedent to that given in the 6th and 8th (he means the 5th and 7th in the Greek Text) Definitions of the 5th book, Dr. Barrow has so fully answered it that nothing further need be added.

"14. Tho Mr. Thomas Simpson is a very able Mathematician, yet he is very much displeased at the high encomiums and extravagant commendations that have been lavished on this 5th book of Euclids; and adds that 'this superb fabrick of proportions, reared 'with so much art, stands upon a tottering foundation' but Dr. Barrow who gives it the highest Encomiums, and who is the person, I believe, he chiefly intends here, was, as is well known, an exceeding modest man, and never launched out too far in behalf of opinions he had adopted; he was on every account, one of the best judges of this affair, and with respect to such objections as have been here taken notice of against Euclid's Definition says in Page 297 of his *Lect: Mathem*: that this definition 'nisi machinis impulsa validioribus in æternum persistet inconcussa.'"

I have not observed in Simson's correspondence with Nourse, any allusion to this paper. It does not, indeed, follow that because no chiasm appears in the letters of this period between them, by reference to missing ones in subsequent letters, that *all* the letters which Simson wrote to Nourse (the one, specified formerly, excepted) are preserved. It is not unlikely that this paper was written soon after Simson's receipt of Simpson's book; as much to satisfy Nourse with his purchase of Simson's copyright as anything else*. One or two passages of the *ad captandum* kind would seem to bespeak such a purpose. Why else the sneer about the able "mathematician" at the opening of art. 14, as a preliminary to his being so strongly contrasted with Dr. Barrow for modesty and learning? The character of the paper is, on the whole, marked by a *hauteur* towards Simpson, that is only excusable in the writer, from a consideration of his age and the profound respect with which he had so long been treated by his own

* It is a remarkable circumstance, that whilst Simson's Euclid is the universally-adopted text-book in geometry in England, it is almost as universally discarded in the Scottish schools and colleges—even in Glasgow itself. In Ireland, Elrington's edition is used; and on the Continent, the Elements is only viewed as a work of learned curiosity, and quoted, where quoted at all, for the purpose of animadversion. The honour of a prophet in his own country is here verified indeed! I have often thought, whilst reflecting upon this, that the maintenance of the preference for Simson's edition in this country was due to the earnest manner in which its superiority was urged at Cambridge by Dr. Robert Smith, and the respect paid to Nourse's known good judgement at Oxford. Probably, but for this, Thomas Simpson's Elements (the best, perhaps, we yet possess, which does not follow close in the wake of Euclid) might have now held the same position in this country that Legendre's does in France; at least if our national propensities to admire everything antique should not have given rise to some new translation with its distinguishing variations.

little circle in Glasgow. Simson was there, what Johnson was in his "Club" in Fleet Street.

Simpson is the only author referred to by Simson, who was both British and unadorned by academic titles; and he is introduced into the first and second editions of the Euclid almost by inevitable necessity. Throughout, he is rather a social *parvenu* than a scientific brother. In many of the views of the preceding paper I concur: still I cannot but regret the tone in which some of them are expressed. The mode, too, in which Dr. Trail refers to Simpson is in too strict keeping with a feeling of contempt towards "those whose works had been criticised in the Doctor's notes;" the reference applying, as far as I have been able to ascertain, to Thomas Simpson only. No other author of the time is mentioned, or even alluded to, in Simson's Notes, and indeed no other work on the Elements of Geometry was published during the interval in question. The first edition of Emerson's was in 1763, and I find no allusion there to Simson or to Simpson either.

I have more particularly alluded to the matters in the preceding paragraph for the purpose of remarking that if a mathematician of Simpson's eminence (and I may add of Simpson's European reputation too) could be thus *sifflé* by "titled scholars," we cannot wonder at the general neglect with which the host of geometers in humble life, during the last century and first quarter of the present one, have been treated by academic bodies*. Few of my readers have the least idea of the existence of a body of men who, through several successive ages, have cultivated geometry with an ardour that is probably unexampled, and with a degree of success commensurate with that ardour. Their reward, as well as their patrimony, was poverty; and their fame was limited to their own narrow circle.

* As a specimen of even a somewhat recent manifestation of this spirit, I may quote Dr. Cresswell's treatise on Maxima and Minima. He says, "the Elements of Thomas Simpson contain a series of propositions on the Maxima and Minima of Geometrical Quantities, in which there is not much that is original" (p. 5). I confess myself unable to find in any writer antecedent to Simpson, a large *relative* portion of the propositions in his book, and I cannot accede to the dictum of Dr. Cresswell, except in the qualified sense of the chapter itself being a 'little' one—barely fifteen pages. But when a writer thus becomes critical upon the subject of *originality* in others, we have a right to test him by the same criterion. What is there "new" in Dr. Cresswell's own treatise, either as to principle, method of development, or final result? Certainly a finer opportunity for the production of a truly classical work on the subject was never thrown away by any writer. Cresswell's volume on this one subject is really larger than Simpson's entire treatise on geometry generally. No man can do much in fifteen pages, but any competent person might do a good deal in 273.

Their works, however, live after them : but *at present*, chiefly forming portions of books of high pretensions, and bearing other names than theirs on the title-pages. These were men of the school of Thomas Simpson; their career was marked out by him; and their tastes were formed upon the models which he bequeathed to them. It may be safely affirmed that, as regards geometry generally, and geometrical construction especially, no works in our language furnish so many beautiful, varied, and instructive exemplars as the three works of Simpson, viz. his “Elements of Geometry,” the supplementary part of his “Elements of Algebra,” and the second part of his “Select Exercises.” It is much to be regretted that Dr. Stewart’s *Propositiones Geometricæ* was not added to the scanty libraries of these able geometers, by its being published in their own, instead of a dead language. That day of learned foppery is, however, gone by, when a man who wishes to publish a work on science must claim his title to “respectable birth, parentage, and education,” by his Latin prose composition, before he can obtain the notice of the *dilettanti* of the so-called literary and scientific world. Justice, however, will yet be done to these men, humble artisans, excisemen, and country-schoolmasters though most of them were. The able analyses of their little duodecimo annuals which is in progress in the *Mechanics’ Magazine*, by Mr. Wilkinson of Burnley, will do much towards effecting this purpose; and I should hope that book-manufacturers will in the end be compelled to bow to the force of public opinion, so far as to give at least some distinct acknowledgement of the sources whence they obtain the materials of their works. A work might be pointed out—a work which has passed through several large editions, and produced to its “author” large sums of money—which is made up *wholly* out of the periodicals written by these men—without, in any single case, more than a verbal alteration in the solutions, and very rarely even so much as that. Yet there is not the least hint given as to whence these beautiful investigations were taken; nor indeed any marked indication that they did not “drop from the clouds,” or were the honest produce of the mind of him whose name adorns the title-page.

Shooters’ Hill, Oct. 27, 1849.

LXIII. *Illustrations of a Method for computing Magnetic Declination, on the principle proposed by Professor Gauss.*
By SAMUEL BESWICK*.

THROUGH the kind suggestion of the Astronomer Royal, I have been induced to examine attentively the elegant and profound Essay of Professor Gauss on the subject of Terrestrial Magnetism. For some time I have used a method for computing magnetic declination, differing from the one proposed by this eminent magnetician, though the involved principle is precisely identical: but this fact was wholly unknown until very recently. The method referred to has been applied with varied success to more than *three hundred places*, their localities being distributed over the four continents and the principal oceans. A specimen of its application to Greenwich, its *antipode* Sydney, and St. Helena, is presented in this communication for the examination of your readers. The principle of Prof. Gauss's method for computing magnetic declination is given in these words (Taylor's Scientific Memoirs, vol. ii.):—

“Consequently there are on the earth *only two magnetic poles*, apart from the possible case of local exception spoken of in art. 13.”—P. 223.

“The two extreme values of V correspond in this point of view to *two points*, inclosed by the zones, at which the horizontal force is $=0$, and where therefore the whole magnetic force can only be vertical; *these points are termed the magnetic poles of the earth.*”—P. 195.

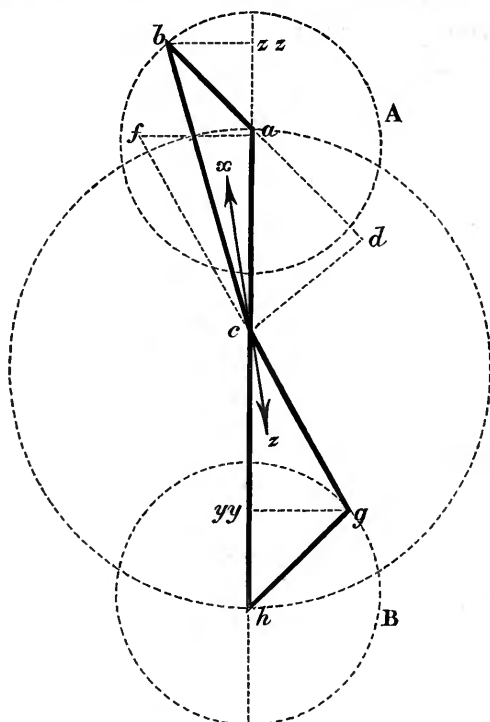
“We proceed to *develope the mode of submitting them to calculation*. On the surface of the earth V becomes a simple function of two variable magnitudes, for which we will take the geographical longitude reckoned eastward *from an arbitrary first meridian*,—and the distance *from the north pole of the earth* (a complement of geographical latitude).”—P. 199.

“Resolving the horizontal magnetic force into two portions, one of which, X , acts in the direction of the geographical meridian, and the other, Y , perpendicularly to that meridian, —and considering X as positive when directed towards the north, and Y as positive when directed towards the west.”—P. 200.

I propose to give a demonstration of the above method, essentially differing from the one given by the Professor, and such as I have used for years.

* Communicated by the Author.

Explanation of the diagram.—The letters a, h represent the situation of the earth's poles; the capital letters A, B represent the two orbits of the magnetic points of convergence or poles; and b, g represent those poles.



Proposition.—Given the latitude and longitude of each magnetic pole, or point of convergence of the horizontal force, and of the place of observation, to find the declination of the needle at the said place.

Mean Declination at Greenwich for the present year 1849.

(1.) N. lat. $51^{\circ} 29'$: comp. $38^{\circ} 31'$.
long. $0^{\circ} 0'$.

(2.) Lat. and long. of N. magnetic pole.
N. lat. 70° : comp. 20° .
W. long. 91° .

(3.) Obtain the value of $z z a$ trigonometrically thus: as
 $b z z a : b a :: a b z z : z z a$.

Or thus:

sine	20° 0'	953405
sine	1° 0'	824186
sine	20'	<u>777591</u>

Throughout the computation we shall consider it useless to subtract the radius, which only leaves the result *without the first figure*. The middle sine 1° is obtained thus:

$$91^{\circ} \text{ (as above) } - 90^{\circ} = 1^{\circ}.$$

(4.) Lat. and long. of S. magnetic pole.

$$\text{S. lat. } 75^{\circ} 5' : \text{comp. } 14^{\circ} 55'.$$

$$\text{E. long. } 155^{\circ}.$$

(5.) Obtain the value of *yyh* in the same way as in item third.

	155° - 90° = 65°.	
sine	14° 55' 941063
sine	65° 0' <u>995728</u>
sine	13° 29' 936791

(6.) Obtain the value of *zxc* thus: add, or subtract, as the case may be, the value of *zxa*, found in item third, to the complement of latitude or *ac*.

$$38^{\circ} 31' + 20' = 38^{\circ} 51'.$$

(7.) Allow 20'' to every degree *without* the orbit of the magnetic poles, for the variation of intensity, and 1' to every degree within the orbit.

$$\begin{array}{r} 38^{\circ} 51' \times 20'' = 12' 57'' \\ 20^{\circ} 0' \times 1' = 20' 0'' \\ \hline 32' 57'' \end{array}$$

(8.) Obtain the distance *zyy* thus:

$$zxa + ah + hyy;$$

or

$$180^{\circ} + 20' + 13^{\circ} 29' = 193^{\circ} 49'.$$

(9.) Correct for the earth's rotundity in latitude, thus:

$$ah^2 : zyy^2 :: 32' 57'' : 38' 12''.$$

These minutes are for every *ten degrees* of the distance *zxc*; hence

$$38^{\circ} 51' \times 38^{\circ} 12' \div 10 = 2^{\circ} 28'.$$

This number is used for the correction of latitude, thus:

$$20^{\circ} + 2^{\circ} 28' = 22^{\circ} 28'.$$

(10.) Correct for the earth's rotundity in longitude. At a distance of 90° from each pole all angles vanish; I therefore take the least and greatest distances of the magnetic pole, and proceed in a duplicate ratio, taking 90° as a basis, thus:

$$70^\circ : 110^\circ :: 51^\circ 29' : 59^\circ 30' :$$

which is the same as saying, as the square of the least distance of the magnetic pole is to the greatest distance, so is the square of the latitude of Greenwich to the number required.

This number must be adapted to the angle of longitude by the correcting number in the middle clause of item ninth.

$$90^\circ : 59^\circ 30' :: 2^\circ 28' : 1^\circ 37'.$$

This number is used for the correction of longitude; thus, in the present case,

$$91^\circ - 1^\circ 37' = 89^\circ 23'.$$

(11.) Having made these corrections, now obtain the trigonometrical values of the side and angle, bc and bca .

First find the side dc , thus: as the radius is to the sine ac ($38^\circ 31'$), so is the sine dac ($89^\circ 23'$) to the sine dc ; or thus:

sine $38^\circ 31'$	979431
sine $89^\circ 23'$	999997
sine $38^\circ 31'$	979428

Find the side da , as follows: as the tangent to the complement ac ($38^\circ 31'$) is to the whole sine, so is the sine of the complement cad ($89^\circ 23'$) to the tangent ad ($29'$); or thus:

sine of comp. and rad. $89^\circ 23'$. .	1803194
tang. to comp. . . $33^\circ 31'$. .	1009914
tan $29'$. .	793280

The side bc may now be found in the following manner. Obtain bd by adding ba and ad , thus:

$$22^\circ 28' + 29' = 22^\circ 57';$$

then bc results from the following proportion: as the radius is to the sine of the complement dc ($38^\circ 31'$), so is the sine of the complement db ($22^\circ 57'$) to the sine of the complement bc ($43^\circ 54'$). Or logarithmically thus:

sine to comp. $38^\circ 31'$	989344
sine to comp. $22^\circ 57'$	996419
sine to comp. $43^\circ 54'$	985763

This is the value of the side bc .

(12.) Now find the angle bca , thus: as the sine bc is to the sine bac , so is the sine ba to the sine bca . Or thus:

sine $22^{\circ} 28'$	958223
sine $89^{\circ} 23'$	999997
		<u>1958220</u>
sine $43^{\circ} 54'$	984098
sine $33^{\circ} 26'$	<u>974122</u>

which is the angle *bca*.

Inasmuch as the process for finding the values of the sides and angles, and the corrections for the earth's rotundity in latitude and longitude, is precisely identical when applied to the southern hemisphere, it would be useless to repeat the explanations and details of the process. But in order to facilitate the computation, and yet avoid repetition, *corresponding items* shall be arranged under corresponding numbers.

$$(6.) \quad 38^{\circ} 31' - 13^{\circ} 29' = 25^{\circ} 2'.$$

$$(7.) \quad 25^{\circ} 2' \times 20'' = 8' 20''$$

$$14^{\circ} 55' \times 1' = 14' 55''$$

$$\underline{23' 15''}$$

$$(8.) \quad 180^{\circ} + 20' + 13^{\circ} 29' = 193^{\circ} 49'.$$

$$(9.) \quad 32400 : 37564 :: 23' 15'' : 26' 57''.$$

$$26' 57'' \times 25^{\circ} 2' \div 10 = 1^{\circ} 7'.$$

$$14^{\circ} 55' + 1^{\circ} 7' = 16^{\circ} 2'.$$

$$(10.) \quad 75^{\circ} 5^{1/2} : 104^{\circ} 55' :: 51^{\circ} 29^{1/2} : 49^{\circ} 19'.$$

$$90^{\circ} : 49^{\circ} 19' :: 1^{\circ} 7' : 36'.$$

$$155^{\circ} + 36' = 155^{\circ} 36' : \text{compl. } 24^{\circ} 24'.$$

$$(11.) \quad \begin{array}{ll} \text{sine } 38^{\circ} 31' & 979431 \\ \text{sine } 24^{\circ} 24' & 961606 \\ \text{sine } 14^{\circ} 54' & \underline{941037} \\ \text{sine of comp. and rad. } 24^{\circ} 24' & . . . 1995937 \\ \text{tang. to comp. } & . . . 38^{\circ} 31' . . . \underline{1009914} \\ \text{tang. } & 35^{\circ} 56' . . . 986023 \end{array}$$

$$35^{\circ} 56' - 16^{\circ} 2' = 19^{\circ} 54'.$$

$$\begin{array}{ll} \text{sine to comp. } 14^{\circ} 54' & 998515 \\ \text{sine to comp. } 19^{\circ} 54' & \underline{997326} \\ \text{sine to comp. } 24^{\circ} 41' & 995841 \end{array}$$

The complement is $155^{\circ} 19'$.

(12.)	sine $16^{\circ} 2'$	944122
	sine $24^{\circ} 24'$	961606
			<hr/> 1905728
	sine $24^{\circ} 41'$	962076
	sine $15^{\circ} 51'$	<hr/> 943652

which is the southern angle.

(13.) Having obtained the two sides and angles required by the theory of Gauss, we now proceed to find the value of the angle bcx formed by the needle. As the sum of the sides $bc + cg$ ($43^{\circ} 54' + 155^{\circ} 19' = 199^{\circ} 13'$) is to the sum of the angles or $bca + gch$ ($33^{\circ} 26' + 15^{\circ} 51' = 49^{\circ} 17'$), so is the north side bc ($43^{\circ} 54'$) to the angle bcx ($10^{\circ} 51'$). Or more clearly thus:

Sides.	Angles.
$43^{\circ} 54'$	$33^{\circ} 26'$
$155^{\circ} 19'$	$15^{\circ} 51'$
<hr/>	<hr/>
$199^{\circ} 13'$	$49^{\circ} 17'$
	$:: 43^{\circ} 54' : 10^{\circ} 51'$

Hence if bcx be subtracted from bca , there will remain the angle xca which the needle makes with the meridian of Greenwich, in other words the declination.

$$\begin{array}{r} 33^{\circ} 26' \\ 10^{\circ} 51' \\ \hline 22^{\circ} 35' \text{ W.} \end{array}$$

which is the mean west declination at Greenwich for the present year 1849.

Mr. James Glaisher, of the Royal Observatory, informed me, June 27, 1849, "*that the average value of the W. declination at Greenwich, at present, is about $22^{\circ} 35'$.*"

Calculation of the Mean Declination at St. Helena for the present year 1849.

(1.) S. lat. $15^{\circ} 55'$: comp. $74^{\circ} 5'$.
W. long. $5^{\circ} 43'$.

(2.) Lat. and long. of N. magnetic pole.

N. lat. 70° : comp. 20° .
W. long. $91^{\circ} - 5^{\circ} 43' = 85^{\circ} 17'$.

(3.) $90^{\circ} - 85^{\circ} 17' = 4^{\circ} 43'$.

sine $20^{\circ} 0'$	953405
sine $4^{\circ} 43'$	891502
sine $1^{\circ} 37'$	<hr/> 844907

(4.) Lat. and long. of S. magnetic pole.

S. lat. $75^{\circ} 5'$: comp. $14^{\circ} 55'$.

E. long. $155^{\circ} + 5^{\circ} 43' = 160^{\circ} 43'$.

(5.) $160^{\circ} 43' - 90^{\circ} = 70^{\circ} 43'$.

sine $14^{\circ} 55'$ 941063

sine $70^{\circ} 43'$ 997492

sine $14^{\circ} 4'$ 938555

(6.) $74^{\circ} 5' + 1^{\circ} 37' = 75^{\circ} 42'$.

(7.) $75^{\circ} 42' \times 20'' = 25' 14''$

$20^{\circ} 0' \times 1' = 20' 0''$

45' 14''

(8.) $180^{\circ} - 1^{\circ} 37' + 14^{\circ} 4' = 192^{\circ} 27'$.

(9.) $32400 : 37037 :: 45' 14'' : 51' 39''$.

$51' 39'' \times 75^{\circ} 42' \div 10 = 6^{\circ} 30'$.

$20^{\circ} + 6^{\circ} 30' = 26^{\circ} 30'$.

(10.) $70^{\circ} : 110^{\circ} :: 15^{\circ} 55^{1/2} : 5^{\circ} 40'$.

$90^{\circ} : 5^{\circ} 40' :: 6^{\circ} 30' : 24'$.

$85^{\circ} 17' + 24' = 85^{\circ} 41'$.

(11.) sine $74^{\circ} 5'$ 998302

sine $85^{\circ} 41'$ 999877

sine $73^{\circ} 31'$ 998179

sine of comp. and rad. $85^{\circ} 41'$. . 1887661

tang. to comp. . . . $74^{\circ} 5'$. . 945511

tang. $14^{\circ} 47'$. . 942150

$14^{\circ} 47' + 26^{\circ} 30' = 41^{\circ} 17'$.

sine to comp. $73^{\circ} 31'$ 945292

sine to comp. $41^{\circ} 17'$ 987590

sine to comp. $77^{\circ} 41'$ 932882

(12.) sine $26^{\circ} 30'$ 964953

sine $85^{\circ} 41'$ 999877

1964830

sine $77^{\circ} 41'$ 998989

sine $27^{\circ} 6'$ 965841

which is the northern angle.

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$$(6.) \quad 74^{\circ} 5' + 14^{\circ} 4' = 88^{\circ} 9'.$$

$$(7.) \quad \begin{array}{r} 88^{\circ} 9' \times 20'' = 29' 23'' \\ 14^{\circ} 55' \times 1' = 14' 55'' \\ \hline 44' 18'' \end{array}$$

$$(8.) \quad 180^{\circ} - 1^{\circ} 37' + 14^{\circ} 4' = 192^{\circ} 27'.$$

$$(9.) \quad 32400 : 37037 :: 44' 18'' : 50' 38''.$$

$$50' 38'' \times 88^{\circ} 9' \div 10 = 7^{\circ} 26'.$$

$$14^{\circ} 55' + 7^{\circ} 26' = 22^{\circ} 21'.$$

$$(10.) \quad 75^{\circ} 5^{1/2} : 110^{\circ} :: 51^{\circ} 29^{1/2} : 4^{\circ} 42'.$$

$$90^{\circ} : 4^{\circ} 42' :: 7^{\circ} 26' : 23'.$$

$$160^{\circ} 43' - 23' = 160^{\circ} 20' : \text{comp. } 19^{\circ} 40'.$$

$$(11.) \quad \begin{array}{rcl} \text{sine } 74^{\circ} 5' & . & . & . & 998302 \\ \text{sine } 19^{\circ} 40' & . & . & . & 952705 \\ \text{sine } 18^{\circ} 53' & . & . & . & 951007 \end{array}$$

$$\text{sine of comp. and rad. } 19^{\circ} 40' \quad . \quad . \quad 1997390$$

$$\text{tang. to comp.} \quad . \quad . \quad 74^{\circ} 5' \quad . \quad . \quad 945511$$

$$\text{tang.} \quad . \quad . \quad . \quad . \quad 73^{\circ} 9' \quad . \quad . \quad 1051879$$

$$73^{\circ} 9' + 22^{\circ} 21' = 95^{\circ} 30' : \text{comp. } 84^{\circ} 30'.$$

$$\text{sine to comp. } 18^{\circ} 53' \quad . \quad . \quad . \quad 997597$$

$$\text{sine to comp. } 84^{\circ} 30' \quad . \quad . \quad . \quad 898157$$

$$\text{sine to comp. } 84^{\circ} 48' \quad . \quad . \quad . \quad 895754$$

The complement to be used is $95^{\circ} 12'$.

$$(12.) \quad \begin{array}{rcl} \text{sine } 22^{\circ} 21' & . & . & . & 958008 \\ \text{sine } 19^{\circ} 40' & . & . & . & 952705 \end{array}$$

$$1910713$$

$$\text{sine } 84^{\circ} 48' \quad . \quad . \quad . \quad 999821$$

$$\text{sine } 7^{\circ} 23' \quad . \quad . \quad . \quad 910892$$

$$(13.) \quad \begin{array}{rcl} \text{Sides.} & & \text{Angles.} \\ 77^{\circ} 41' & 27^{\circ} 6' \\ 95^{\circ} 12' & 7^{\circ} 23' \end{array}$$

$$172^{\circ} 53' : 34^{\circ} 29' :: 77^{\circ} 41' : 15^{\circ} 29'.$$

$$7^{\circ} 23'$$

$$15^{\circ} 29'$$

$$22^{\circ} 52' \text{ W.}$$

which is the mean west declination at St. Helena for the present year 1849.

Mr. Glaisher, of Greenwich Observatory, kindly informs me, that "*at St. Helena, 1845, it was $23^{\circ} 32'$ W., decreasing $8'$ yearly.*"

Hence in 1849 it would have decreased about $32'$, leaving the declination about 23° W., which is only $8'$ more than our computation. Even this would probably diminish, if not entirely vanish, could we obtain the mean declination for the present year.

Manchester, Nov. 23, 1849.

LXIV. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 391.]

Nov. 9, **A**PPPEARANCE of Saturn's Ring, &c. in the Equatoreal 1849. of Cambridge, United States. By Professor Bond.

"During the period of the disappearance of Saturn's ring in 1848, we often noticed breaks or inequalities in the ring, such as would arise from irregularities in its structure, were the matter of which it is composed unequally distributed in its different parts.

"Something of this kind has often before been observed, but hitherto only on the *illuminated* side of the ring. That similar appearances present themselves also on the *unilluminated* side is a new feature, important in its bearing on the true explanation of these phenomena,

"From June to September 1848 the light reflected from the edge of the ring (the only part then visible), instead of being uniformly distributed over a single line, was interrupted on each side of the planet by spaces of some seconds in breadth, where it was barely possible to trace the continuity of the edge. These inequalities were sufficiently abrupt to render it difficult to distinguish at first sight between them and the somewhat similar effects produced by either of the small satellites being projected upon the ring; their presence could only be detected by their motion, of which the indications were usually decisive in 15 or 20 minutes. On the other hand, the irregularities of the ring always retained one fixed position with reference to the ball, as long as the earth remained elevated above the *southern unilluminated* surface.

"The same appearances were again presented between September and January 1849, while the earth was elevated above the *northern unilluminated* surface, in all this time retaining one fixed position with respect to the ball.

"It is an unavoidable inference from our observations that these breaks in the illumination of the edge do not rotate about the globe of Saturn; a result perfectly in accordance with what Schroeter has established with regard to the corresponding irregularities on the *illuminated* side.

"The fact of these inequalities always retaining one unaltered position may be explained without precluding the possibility of a rapid rotation of the rings, by attributing them to the reflexion of the solar rays from their inner edges. There seems to be no other way of accounting for their being seen on both surfaces, both when illuminated and when turned away from the sun.

"The first reappearance of the ring took place between August 31^d.78, Greenwich M.T., and Sept. 3^d.80; the second disappearance between Sept. 12^d.80 and Sept 13^d.62; and the final reappearance in 1849 between Jan. 18^d.47 and Jan. 19^d.43.

"On two or three occasions in the past year we have enjoyed the sight of all the eight satellites at once.

"Our observations for a year or two past on the variations of the brightness of Jupiter's satellites afford some curious results. Attention was first drawn to the subject in observing a transit of the third satellite on the 6th of January, 1847. A few minutes after its entrance the satellite was visible on the disc just within the limit of the planet, being brighter than the surrounding surface; soon after a dark spot, supposed to be the satellite, could just be discerned in its place. The telescope used was an achromatic of $2\frac{3}{4}$ inches aperture, of excellent quality, but not powerful enough to enable us to decide whether or not the spot was really the satellite.

"On the 28th of January, 1848, during the transit of the shadows of the first and third satellites, the third satellite itself was seen with the great refractor under very beautiful definition, as a black spot between the two shadows, and not to be distinguished from them except by the place it occupied. It was smaller than its shadow in the proportion of 3 to 5, not duskish simply, but quite black like the shadows. On the 11th of March it was again seen dark on the disc, smaller than its shadow.

"On the 18th of March we watched the entire transit. At the first internal contact the satellite was distinctly seen on the disc, *brighter* than Jupiter, though it had entered on a bright channel south of one of the great equatorial belts; 20 minutes after it had become nearly of the same brightness with the planet, so as to be barely perceptible, yet still whiter than the surrounding surface. While watching it with close attention, a minute dark speck suddenly made its appearance, in the place of the satellite, increasing very rapidly till it occupied a space of about one second of arc in diameter, quite black and nearly round, though an irregularity of shape was suspected. Remaining thus for about two hours, the darkness gradually lost its intensity, and quite disappeared before the satellite left the disc. Something of this nature we have always observed to accompany a transit of this satellite. The first and fourth satellites we have also seen black or dusky on the disc, but the former has once or twice crossed without our detecting any change. The spots are always less than the shadows, but have appreciable diameters, and make their appearance after the entrance of the satellite upon the limb of Jupiter. Changes of relative brightness are constantly going on; the feeblest, on the average, being the fourth, and the brightest usually the third.

"It would be well, as soon as Jupiter reaches a convenient position, say during the ensuing winter and in the spring of 1850, for observers generally to record their estimates of the relative brightness of the satellites as often as possible. The labour of doing this will be but trifling, and may lead to the discovery of the laws of these singular phenomena.

"We have observed Schweitzer's comet twice since its reappearance.

"From the circles of the great equatoreal compared with B. A. C. No. 1634, we have—

Cambridge M.T.				Comet's R.A. 1849.0				Comet's Decl. 1849.0			
1849.	h	m	s	h	m	s					
Aug. 24	15	37	30	4	45	0.4		—27	14	14	
26	15	38	42	4	41	5.4		—27	17	45	

"With the micrometer we have the following places :—

Cambridge M.T.											
1849.	h	m	s								
Aug. 24	15	37	30	Comet follows *	in	0	34.75	Comet N. of *	by	7	36.5
26	15	38	42	precedes *	by	3	20.35	N. of *	by	4	5.7

* is Lacaille 1613, whose place seems to be erroneous in N. P. D.

"The star Lalande 9167, of the 7.8 magnitude, is missing."

The Astronomer Royal exhibited an instrument for performing arithmetical multiplications and divisions, constructed under the direction of William Bell, Esq., Coronation Road, Bristol. The President remarked, that for mere exhibition of the significant figures produced by a single multiplication or division, to a certain degree of accuracy, nothing could be more convenient than the common sliding rule containing two similar scales, one fixed and the other moveable, in which the lengths corresponding to the numbers represented are the logarithms of those numbers corresponding to a certain modulus. In the scales of this kind in common use at the Royal Observatory, the distance from 1 to 10 upon the scale is about 12 inches; and with these dimensions, a product or quotient will be accurate, with the roughest degree of attention, to $\frac{1}{300}$ part; an accuracy which suffices for a vast amount of the small calculations in an observatory. And with proper scales for trigonometrical functions, the problems of plane and spherical trigonometry can in most cases be easily solved. These inconveniences, however, attend it:—1st. It cannot be conveniently applied to multiply three or more quantities, or in general to exhibit a product of even two which is given in a different denomination; as when a sine is given by the product of two tangents; this failure, however, may usually be remedied by the use of two parallel sliders. 2nd. When used to multiply numbers, it gives no information as to the place of the decimal point in the product. It is to remove some of these inconveniences, but especially the last, that Mr. Bell has constructed his instrument.

The logarithmic spaces in Mr. Bell's machine are arranged upon a circle, a single series of numbers from 1 to 10 occupying the entire circumference. This construction, as is well known, may be used in the same way as the common sliding rule; but, like it, it gives no

information as to the decimal point. But the revolving circle is connected by toothed wheel-work with two smaller circles, each of which performs one revolution for ten revolutions of the principal circle, and each of which has upon its circumference ten series of logarithmic numbers. These small circles, then, give us the means of defining the decimal place, in multiplicand, in multiplier, and in product. Thus, the successive figures 1 upon one circumference may stand for $\cdot 00001$, $\cdot 0001$, $\cdot 001$, $\cdot 01$, $\cdot 1$, 1 , 10 , 100 , 1000 , 10000 . The rotating index of each circle is adjustable; there are also adjustable indices on the fixed frame. Suppose, then, we had to multiply $27\cdot 42$ by $332\cdot 6$, we should, both in the principal circle and in one of the smaller circles, set the 1 of the moveable circle or index to $27\cdot 42$ on the fixed circle; but there would be this difference, that on the principal circle we have no respect to the decimal point, whereas on the smaller circle we should set to the 2742 following the 10 . Then for $332\cdot 6$ we should turn the principal circle from 1 to 3326 , but we should also turn it two complete revolutions, which would carry the index of the second small circle from 1 to the 3326 following 100 . Then the figures of the product will be exhibited very accurately on the principal circle, and much less accurately on the first of the small circles, but they would be found in that series of numbers which follows 1000 , and the place of the decimal point would thus be exactly defined.

For the multiplication of three or more numbers, it is necessary to plant moveable indices after performing the first products; this process, however, is much less convenient than that with two sliding scales in the sliding rule.

The Astronomer Royal remarked, that in his opinion this construction is too expensive and too cumbrous to be extensively used. But he wished much to call the attention of members of the Society to the use of the sliding rule, and to the peculiar defect which he had indicated; and to represent to them the great value of any simple construction, which, while preserving the other advantages of the sliding rule, would effectually remove that defect.

Mr. Drach suggests that a calculating machine, exhibited by Mr. F. Schiereck in 1837 (the construction of which was concealed by the inventor, who wanted 700*l.* for his secret), might probably be a particular modification of the sliding rule applied to a circular form. He also suggests the addition of other concentric circles to Mr. Bell's scale, in which the trigonometric or other logarithmic numbers might be laid down.

The Astronomer Royal gave an oral statement of the progress made by Lord Rosse in the mounting of his 6-feet speculum.

In a lecture delivered on this subject by the Astronomer Royal in November 1848, a detailed account was given of the distortions produced in the speculum by using the telescope at different inclinations to the horizon, and of the explanation of these distortions suggested by Lord Rosse, and also of the nature of the remedial measures tried by Lord Rosse while the Astronomer Royal was at Parsonstown. It will be sufficient to recall here, that when the edge of the speculum

rested against short iron pillars fixed in the breech-piece of the telescope, the firm hold produced by friction of the edge of the speculum on the pillars, combined with the varying elasticity of the triangular lever-supports in different inclinations of the telescope, caused distortion of the mirror; and that when, to avoid this friction, the mirror was suspended (as regarded its edge-bearing), either by a semicircular hoop or by a chain, a small difference in the edgewise pressure, depending on a difference of inclination of the telescope, threw some of the points of the supporting levers out of bearing, and distortion of the mirror was produced. This latter fault arose from the circumstance that the mirror could not slip freely over the points of the supporting levers. To remedy this, the following arrangement is now made. Each of the plates resting on the 27 supporting points, instead of being partially attached to the mirror by a layer of felt and pitch, is completely separated from it, to the distance of about $1\frac{1}{4}$ inch, and the speculum rests upon each plate by three turned brass balls at the three angles of the plate; so that the whole surface of the speculum is now supported by eighty-one brass balls. Each of these balls has a fine wire passing through a small hole in the plate, and kept in tension by a weak spring on the opposite side; this prevents the ball from rolling away when the mirror is detached, but allows entire freedom of motion to the ball, to the extent of about one inch in any direction. Lord Rosse has reason to think that this construction is perfectly successful for its object. He has already found that the speculum may be moved laterally half an inch without the smallest discoverable distortion. Before the balls were used, when the speculum was moved laterally $\frac{1}{30}$ th of an inch, vision was destroyed.

Lord Rosse thinks it, however, desirable to arrange the edgewise support, so that as little as possible may be trusted to the motion of the balls. The lower semicircle of the edge is now to be grasped by a strong iron hoop, very neatly fitted to it; and the upper semicircle is to have a thin hoop furnished with a drawing-screw or contracting screw, merely for the purpose of bringing the strong hoop constantly into fair contact with the edge of the mirror. And the ends of the strong hoop (which are at the extremity of a horizontal diameter of the mirror) are to be supported by rods, attached to a horizontal bar which rests on the two upper pillars of the breech-piece, with the utmost freedom of motion; so that the mirror will be supported edgewise by jimbals of the most perfect construction. Lord Rosse hopes that, with this arrangement, in combination with the support on the balls already described, the mounting of the mirror will be sensibly perfect.

Lord Rosse had also communicated to the Astronomer Royal some remarks upon the process of grinding large mirrors. With mirrors of 3 feet aperture there is not the smallest difficulty. In the mirrors ground and polished by his apparatus, there is no appreciable difference of focal length of the central part and the annulus next the edge; and this result is obtained uniformly. Still, with the 6-feet mirrors there is great difficulty. In all cases, a figure is ob-

tained which will do well for work ; rarely is one obtained which is perfectly satisfactory. This arises in part from the impossibility of testing the mirror while it is under the machine.

Adverting to Mr. Lassell's use of a wooden polisher, Lord Rosse had stated to the Astronomer Royal, that he himself had at first used a wooden polisher, but that he had abandoned it, as there appeared to be abundant evidence that the polisher was continually changing its figure from the absorption of moisture. He considers it totally inadmissible for very large specula. It will, however, probably be necessary, with Mr. Lassell's apparatus, to use a light wooden polisher, because it appears scarcely practicable in that apparatus to apply a counterpoise.

In speaking of the results of observations with the large telescope, Lord Rosse had stated to the Astronomer Royal that the nebula H. 131 exhibited a well-marked spiral structure, and that 2241 has a central hollow.

Some Remarks on Falling Stars by Mr. Lowe*.

Mr. Lowe gives the following epochs when falling stars are said to be abundant :—

April 22^d to 25^d

July 17 .. 26

Aug. 9 .. 11

Nov. 12^d to 14^d

Nov. 27 .. 29

Dec. 6 .. 12

To which he adds from his own observation, Oct. 16^d to 18^d. There are many occurrences in January, but the days do not appear to be fixed.

Mr. Lowe then states by whom observations have been made at the foregoing epochs, and when and where.

The August epoch has been observed every year since 1841, and is said by Mr. Lowe to be the most certain.

The November epoch, 12^d to 14^d, seems also well-fixed, but has in late years been surpassed by the August epoch in brilliancy.

The epochs, Nov. 27^d to 29^d, and Dec. 6^d to 12^d, are somewhat doubtful, so far as Mr. Lowe's observations extend.

The October epoch has been observed in 1843, 4, 6, 7 and 8, but has not, so far as Mr. Lowe knows, been usually considered an epoch.

On referring back the paths of meteors, it is found that a considerable number diverge from the same point in the heavens ; thus, during the July period of the present year, and up to the 9th of August, the paths of the meteors, if produced backwards, would nearly meet at a point to the east of α Cygni.

In 1848, of 80 meteors seen on August 19th, the paths of 55 were recorded ; 23 came from Cygnus, 26 from Cassiopeia, and 6 were discordant. In 1847 out of 13, 5 came from Cygnus and 8 from Cassiopeia. This is confirmed by other observers. It seems very

* Further particulars will be found in the forthcoming Report of the British Association, "Catalogue of Observations on Luminous Meteors," by Professor Powell ; and "Some Remarks on Luminous Meteors," by E. J. Lowe, Esq.

probable that this divergence was merely apparent, and that the meteors were nearly parallel to each other.

On the 16th of August last, Mr. Lowe was surprised to see a number of very small meteors, but brilliant for their size (being only the apparent size of the smallest fixed star on a clear night), which moved more slowly, and occupied a smaller portion of the heavens than on the 10th. He attributes this difference to their greater distance.

On the 10th of last August, Mr. Lowe observed that out of 55 meteors there were 11 cases in which one star followed another in the same track nearly, and after an interval varying from 2 minutes to 15 seconds. When a meteor follows another in the same path, it has invariably been noticed that it also moves with the same speed. From this circumstance, together with the fact that such meteors are frequently very different in size, it may be supposed that the two bodies are connected. If this be so, then a meteor at 10^h 29^m had two attendants. This suggestion, if confirmed, would also show that these bodies are material.

Mr. Lowe conceives that in our present state of knowledge of these curious appearances, we might conveniently arrange them in three classes:—

- 1st. Falling stars which leave luminous streaks behind them.
- 2nd. Stars which do not leave such streaks.
- 3rd. Luminous bodies, with defined discs.

The first probably shine by inherent light, for otherwise it is difficult to account for a luminous streak which lasts several seconds (in some cases even minutes) after the meteor itself has disappeared. The second may shine by reflected light, as described by Sir John Lubbock, and the third are probably atmospheric, as they chiefly move in discordant paths, are various in shape, and not unfrequently change colour.

On the 8th of last August, at 10^h 16^m, a meteor of a conical form, about twice the apparent size of a star of the first magnitude, moved slowly in a horizontal direction from ζ Bootis, about 1° below Arcturus (it left numerous sparks in its track). Here it suddenly disappeared, and reappeared in about 1^s and about 1°½ further on. Thus it had moved in the same track, though invisible. On its reappearance it was neither so large nor so brilliant as before, and gave the impression of a body moving rapidly and nearly directly from the observer. It was orange-red, and was visible about 5^s besides the time of disappearance. The length of its path after its reappearance was about 3°. The disappearance might be accounted for by supposing the meteor to shine by borrowed light, and to have passed through the umbra of some solid body. Sir John Lubbock suggests that it may have passed through the shadow of the earth.

At Castle Lecky, Newtown-Linawady, county of Londonderry, Mr. Webb saw (Nov. 1^d, at 11^h P.M.) in the north, a descending meteor, which having fallen a considerable distance in an inclined straight line diverged suddenly (to the west) on approaching towards a dense cloud. The meteor described a curved path, concave to the cloud, by which it was evidently diverted, and then disappeared.

INSTITUTION OF CIVIL ENGINEERS.

Nov. 27, 1849.—“ A description of the Old Southend Pier-head, and the extension of the pier ; with an inquiry into the nature and ravages of the *Teredo navalis*, and the means hitherto adopted for preventing its attacks.” By Mr. J. Paton.

After describing the form of construction of the old pier-head, and showing the adoption of copper sheathing for protecting it from decay, and the important considerations involved in the attempt to preserve marine structures, the paper explained the ravages committed by marine worms (*Teredo navalis*, *Limnoria terebrans*, and others) on the piles, both above and below the copper sheathing. A general outline of the extension of the pier, and a minute description of the pier-head, were then given ; followed by an investigation of the nature and operations of the *Teredo navalis*, which showed, as a remarkable peculiarity, that no chemical means had hitherto prevented wood from being destroyed by these animals and the *Limnoria terebrans*, whose destructive powers were likewise noticed, as having penetrated between the copper sheathing and the wood at Southend. The operations of the *Teredo*, although most destructive in warm climates, extended themselves to all places, having been found almost in the Polar seas. The chief peculiarities which distinguished the *Teredo* were stated to have been ascertained by minute microscopical investigation, and that woody fibres of an extremely minute nature had been discovered in the body, thus setting at rest the question as to whether the *Teredo* did actually feed upon the wood. It was stated, that the failure of chemical means to preserve timber from destruction by the marine worm was believed to proceed from two causes, namely, of poisonous compounds having no seriously injurious effect upon them, and the sea-water, and other things, decomposing the poisonous ingredients contained in the wood.

In corroboration of the first of these views, accounts of experiments made by Mr. Paton were adduced ; and physiological facts, quoted from the British and Foreign Medical Review, were brought forward to show that cold-blooded animals were much more tenacious of life than those of a higher temperament ; and hence it was argued, that as it required a very large quantity of poison of the most virulent nature to destroy animals of a much higher order than the *Teredo navalis*, it would take a still greater quantity to affect those animals as they existed in their own element ; and it was questioned, under these circumstances, whether wood could ever be so completely and thoroughly saturated as in any degree to affect them.

The corrosive action of the sea-water, its extended influence and constant variableness in different parts of the globe, were then commented on, and some of the various salts held in solution mentioned. It was believed to be impossible to form any general notion of the precise action of sea-water on timber, whether chemically saturated or not, without a series of most minute experiments, and a large body of facts carefully collected in different parts of the globe ; as that which might be advantageously used in the Thames might not be of

the slightest avail in the Tropics, and *vice versa* ; it was thus questioned whether any generally applicable principle could be found for the counteracting of that universal solvent of soluble matter.

The conclusions arrived at were, that the ravages of the marine worm were not prevented by any chemical application, and that nothing but mechanical means could ever prove completely successful : studding with broad-headed nails was considered to be the most effectual remedy, and various authorities were quoted, proving its success. The paper concluded with a list of places where wood, prepared with various chemical ingredients, had been destroyed from various causes.

The discussion commenced by the Dean of Westminster, chiefly remarking on the analogous action of the *Pholas* on stone, was announced for continuation at the next meeting.

Dec. 4.—The discussion on Mr. Paton's paper extended to such a length as to preclude the reading of any original communication. Numerous specimens were exhibited, and commented on, of timber thoroughly perforated by worms ; whilst beside them, under the same circumstances, the "Jarrow wood" from Australia was shown to have remained completely free from injury. The reference to the age of Homer, as an instance of the ancient ravaging habits of the *Teredo*, induced a return to geological questions ; and it was shown, that in the London clay remains had repeatedly been found of timber perforated by sea-worms. The oolite and greensand formations also exhibited petrified wood filled with boring molluscs. This led to the consideration of the formation most likely to withstand the attack of the *Pholas* ; and it was shown that the Portland stone was, from the quantity of silica it contained, least liable to be attacked. The *Pholas* was shown to have been in active operation upon certain rocks from the earliest periods, but never upon Portland stone. Hence it was argued, that kind of stone should be used for breakwaters and other works exposed to the action of the sea.

The early state of the *Teredo* was noticed ; when escaping from the egg, in the shape of a free swimmer, it was drifted about with the tide until it met with a log, a pile, or the side of a ship, to which it attached itself, and making an inroad into it, became a non-locomotive animal of different form and habits, never again to leave the habitation it had burrowed for itself in the body of the timber. The question, of whether the boring operation of the marine worms was carried on by chemical or by mechanical means, was lengthily discussed. The thin shell, covered by its delicate membrane, was instanced as not possessing strength enough to cut away timber ; but it was on the other hand shown, that the shape of the two shells, forming the extremity of the animal, admirably adapted them for powerful cutting or rasping tools, when moved rapidly in a circular direction, as was evidently the case, from the uniformly cylindrical character of the holes. The shells of the *Pholas* were also shown to be used in that manner, and the opinion appeared generally to lean to a mechanical cause for the effects observed. This bearing of the discussion naturally induced remarks upon the ravages of the white

ant of India; which, however, appear to have been little studied, and less understood, as far as attempting to arrest or to prevent its inroads.

The various materials, such as Kyan's corrosive sublimate of mercury, Sir W. Burnett's chloride of zinc, Margary's salts of metals, Payne's combination of muriate of lime and sulphate of iron, forming in the timber an insoluble compound, and Bethell's creosote or oil of coal tar, were discussed. All had their partisans, and were stated to have succeeded and failed under certain circumstances. Specimens of piles from Lowestoft harbour, whose waters were notoriously full of worm, showed that timber in a natural state was in a few months thoroughly perforated by *Teredo* in the centre, and *Limnoria* on the surface; but that piles which had been properly saturated according to Bethell's system, in exhausted receivers, and subjected to such pressure as ensured the absorption of about ten pounds' weight of the creosote, or oil of coal tar, by each cubic foot of the timber, were perfectly preserved from attacks of marine animals of any kind. In one instance a partially "creosoted" pile had a notch cut into it, deeper than the impregnation had extended; a *Teredo* made its entry, and was found to have worked in every direction, until it arrived within the reach of the creosote, when the animal turned away and eventually left the pile. Bethell's system was admitted, by all the speakers, to be that which hitherto, after many years' experience, had afforded the most satisfactory results. Some most conclusive experiments, instituted by Mr. Rendel at Southampton, were stated to have produced the same results; and at Leith all the piles were weighed before and after their saturation, to ensure their absorbing the full allowance of at least ten pounds per cubic foot.

ROYAL SOCIETY.

[Continued from p. 235.]

June 21, 1849.—"On the Anatomy and Affinities of the Family of Medusæ." By Henry Huxley, Esq. Communicated by the Bishop of Norwich, F.R.S.

The author commences by remarking that no class of animals has been so much investigated with so little satisfactory and comprehensive result as the family of *Medusæ* (including under that name the *Medusæ*, *Monostomata* and the *Rhizostomidae*), and proposes in this paper to give a connected view of the whole class considered as organized upon a given type, and an inquiry into its relations with other families. This he has been enabled to do through numerous and peculiar opportunities for the investigation of these animals, enjoyed during a cruise of some months along the eastern coast of Australia and in Bass's Strait*.

The memoir is divided into two sections, of which the first treats of the anatomy of the *Medusæ*, and the second of their affinities.

* Mr. Huxley is Assistant-Surgeon to H.M.S. *Rattlesnake*, now engaged on a surveying voyage conducted by Capt. Stanley on the coasts of Australia and New Guinea.

The organs of the Medusæ are formed out of two distinct membranes—foundation membranes. Both are cellular, but the inner is in general softer, less transparent and more richly ciliated than the outer, but contains fewer thread-cells. The outer is dense, transparent, and either distinctly cellular or developed into a muscular membrane. It may be ciliated or not, and is usually thickly beset with thread-cells, either scattered through its substance or concentrated upon more or less raised papillæ developed from its surface. When the stomach is attached to the disc, the outer membrane passes into the general substance of the disc, while the inner becomes continuous with the lining membrane of the canals. There is a larger or smaller space, termed by the author the "common cavity," between the inner aperture of the stomach and the openings of the canals, with which both communicate. This is the structure of the stomach in the Cryptocarpæ and Phanerocarpæ; in the Rhizostomidæ it is fundamentally the same, but the stomachs are very minute, and collected on the edges and extremities of the ramuscles—a common stem. The Rhizostomes, *quoad* their digestive system, have the same relation to the Monostome Medusæ that the Sertularian Polypes have to the Hydræ, or the Coralline Polypes to the Actiniæ. In consequence of a very irritable contractile membrane surrounding and overlapping the orifices of their stomachs, they are seen with difficulty. This membrane consists of two processes, one from each side of the perforated edge of the branch. In Rhizostoma they generally remain distinct, but in Cephea they are frequently united in front of and behind each aperture so as to form a distinct polype-like cell. In the structure of the disc there exists no difference between the Monostome and Rhizostome Medusæ. The author gives an account of his observations on the minute structure of the disc. The arrangement of the cavities and canals of the disc differs in the different sections. In very many of the Cryptocarpæ there is a circular, valvate, muscular membrane developed from the inner and under edge of the disc. In the Phanerocarpæ such a membrane does not seem to be present, but in Rhizostoma and Cephea it is evidently replaced by the inflexed edge of the disc. In the Cryptocarpæ the marginal corpuscles are sessile upon the circular vessel. They are spheroidal vesicles, containing a clear fluid, and one or more strongly-refracting bodies occasionally included within a delicate cell. The marginal vesicles are placed between the inner and outer membranes of the circular vessel. In the Phanerocarpæ the marginal corpuscles are pedunculated and protected by a semilunar fold. The author describes peculiarities in this part of the organization of Rhizostoma. The excretory orifices, described by Ehrenberg as general in *Medusa aurita*, were not detected by the author in *Cephea ocellata*. Nor does he admit the supposed nerves and intertentacular ganglia of that author to be such.

Paragraphs 29 to 36 are occupied by a minute description of the tentacles of Medusæ.

The generative organs of the three groups of Medusæ are always portions more or less developed of the walls of the system of canals,

and consist of the two "foundation" membranes, in or between which the generative elements, whether ova or spermatozoa, are developed. This the author concludes from his observations on several genera, which he gives in detail, and which add considerably to, and differ in some respects materially from, what has been stated by previous observers. In the ovarium, the two membranes develop between them immense multitudes of ova with a dark granulous yolk and clear germinal vesicle. The ova are attached to the outer surface of the inner membrane. In the testis the inner membrane is produced into a vast number of thick pyriform sacs, which lie between the two membranes, with their blind ends towards the inner surface of the outer membrane; internally, they open each by a distinct aperture on the fine surface of the inner membrane. The contents of the sacs are spermatozoa, and cells in every stage of development towards spermatozoa, which appear to be formed by the elongation of the secondary cells contained in the large cells.

The author's observations lead him to believe that the muscular fibres are always developed in the outer "foundation" membrane. Each fibre in *Rhizostoma* is made up of very small and indistinct fibrils, which are transversely striated. He has not observed any indubitable trace of a nervous system in the *Medusæ*, nor of the so-called blood-vascular system described by Will.

In this section of the memoir the affinities of the *Medusæ* are considered. In their essential characters,—viz. their construction out of two membranes inclosing a variously-shaped cavity; their generative organs being external and variously developed processes of the two membranes; and the universal presence of the peculiar organs called thread-cells,—they present a striking resemblance to other families of Zoophytes, as the Hydroid and Sertularian Polypes, the Physophoridæ and the Diphydæ. The disc of a *Medusa* is represented by the natatorial organ among the Diphydæ and Physophoridæ, but has no homologue among the Hydræ and Sertulariæ. The cell of the Sertularian Polype rather resembles the "bract" of the Diphydæ than the natatorial organ, and the latter family forms a connecting link between the *Medusæ* and the Physophoridæ. Of the two kinds of tentacles in the *Medusæ*, the first is represented in the Physophoridæ and Diphydæ, by the thickenings, richly beset with thread-cells, that frequently occur in the lip of the stomach; in the Sertularian Polypes by the tentacles of the margin of the mouth. The second kind is homologous with the prehensile organs of the *Diphydæ* and *Physophoridæ*, and with the peculiar clavate processes of *Plumularia*. All these organs commence their development as bud-like processes of the two joining membranes. The peculiar clavate organs of *Plumularia* are developed from the common tube independently of the stomach. They have not been hitherto described, and were observed by the author in two species of *Plumularia* dredged at Port Curtis. They were of two kinds, the one attached to the cell of the polype, the other to the pedicle of the ovary. To each species there were three processes of the former kind, two above proceeding from near that edge of the aperture

which is towards the stem, the other below from the front part of the base of the cell. They were conical in one species, club-shaped and articulated in the other, and consisted of an external horny membrane open at the apex, and an internal delicate membrane inclosing a cavity, all these being continuous with the corresponding parts of the stem. At the apex of each, and capable of being pressed through the aperture, lay a number of thread-cells. The second kind of organ was present in the species with conical processes. It consisted of a stem proceeding from the pedicle of the ovary, bearing a series of conical bodies, having the same constitution as those just described; the whole bearing a close resemblance to the prehensile organs of the *Diphydæ*.

The following table exhibits the homologies of the several families, which must be regarded as by no means so distinct as hitherto supposed, but rather as members of one great group, organized upon one simple and uniform plan, and even in their most complex and aberrant forms reducible to the same type.

Stomachs identical in Structure throughout.

<i>Medusæ.</i>	<i>Physophoridae.</i>	<i>Diphydæ.</i>	<i>Sertulariæ.</i>	<i>Hydræ.</i>
Disc	Natatorial organ	Natatorial organ.		
Canals	{ Canals of natatorial organ	{ Canals of natatorial organ.		
Common cavity ..				
Canals of branches (<i>Rhiz.</i>)	{ Common tube	{ Sacculus and common tube	{ Cavity of stem.	
	Bract.....			Polype-cell.
Tentacles, 1.	{ Thickened edge of stomach	Oval tentacles.		
2.	Prehensile organs			Clavate organs ... Tentacles (?)
Generative organs	{ Generative sac	Generative organ..		
	{ Natatorial organ of generative sac ..	{ Natatorial organs (Coryne).		
Marginal vesicle	?	?	?	?

“On the Microscopic Structure of the Scales and Dermal Teeth of some Ganoid and Placoid Fish.” By W. C. Williamson, Esq. Communicated by Edwin Lankester, M.D., F.R.S.

The author commences his paper by stating that the structure and modes of growth of fish-scales have been studied by many observers, especially by Leeuwenhoek, Agassiz, Mandl and Owen. The first of these considered each scale to consist of numerous superimposed laminæ added successively to the inferior surface. This view has been revived, with some important modifications, by M. Agassiz, and especially applied to the scales of ganoid fish; which he showed to consist of laminæ of true bone, usually covered with enamel (*émail*), the latter often resembling the dentine of fishes' teeth. M. Mandl denied that ganoid scales had been formed by such successive additions of laminæ; and Professor Owen also opposed the idea, that they had merely been the result of successively excreted deposition. The author then proceeds to the examination of the scales of the following genera and species:—*Lepidosteus osseus*, *Lepidotus semiserratus*, *L. Mantelli*, and *L. fimbriatus*, *Seminotus rhombifer*, *Pholidotus Leachii*, *Ptycholepis Bol-*

lensis, *Beryx*, *Dapidius orbis*, and *D. granulosus*; all of which appear to be constructed according to a common type—one singular modification of which is seen in *Palæoniscus comptus* and *P. Beaumonti*, and another in *Gyrodus* and *Aspidorhynchus acutirostris*. Still more elaborate complications occur in the scales of the Sturgeon and of *Platysomus parvulus*, the minute structure of which is described. Then follow detailed accounts of another interesting group of structures found in the genera *Megalichthys*, *Holoptychius* and *Diplopterus*, in which the osseous tissues and their superficial coverings are exceedingly beautiful and complicated. The next fish examined is *Macropoma Mantelli* from the chalk. In this the true bony operculum is studded over with dermal teeth, as is also the posterior part of each scale; the portion of the latter, however, which is subjacent to these dermal teeth, is not osseous, but consists of thin laminæ, which do not contain lacunæ. The hollow viscus found in the interior of the *Macropoma*, is shown to be a cylinder of true osseous tissue, of a singular laminated structure full of lacunæ. The author rejects the idea of its having been a stomach, but thinks that it may have served the purpose of an air-bladder.

The structure and arrangement of the dermal teeth from the skin of the Dog-fish are then investigated, and appear to resemble those on the opercular bones and scales of *Macropoma*. Similar teeth are described in the fossil skin of *Hybodus reticulatus*, from the lias of Lyme Regis. In the latter, numerous small granules of calcareous matter, having a concentric laminated structure, have been imbedded in the substance of the soft cutis, under the dermal teeth. The corresponding dermal teeth from the *Raia clavata* are described, and also those covering the snout of the common Saw-fish; as well as the very singular premaxillary bones of the *Cælorhynchus*.

From an examination of the dermal appendages of the fishes thus cursorily enumerated, the author concludes—

That what has hitherto been termed enamel, is in fishes a compound structure, separable into ganoin and kosmine (κόσμιον, to adorn); the former being transparent and laminated, but otherwise structureless, whilst the latter consists of minute branching tubes resembling the dentine of true teeth.

That the kosmine covering the osseous scales of so many ganoid fish, as in *Lepidotus semiserratus*, *Megalichthys Hibberti*, &c., is homologous and identical with the substance composing the dermal teeth of the true placoids, such as the Dog-fish, Thornback, &c., only that, whilst in the former the areolæ of kosmine are aggregated upon bony scales, in the latter they are implanted in the soft integument, without the intervention of any bony matter. It follows from this, that the distinction of "ganoid" and "placoid" is scarcely a physiological one, inasmuch as the scales of many so-called ganoid fish, such as *Dapidius orbis*, *Acipenser*, &c., exhibit little or no trace of either ganoin or kosmine; that in many of the Placoids these substances are very largely developed; and that a series of well-defined links exist, passing through the common Thornback, the common Spotted Dog-fish, *Hybodus reticulatus*, *Macropoma Mantelli*, *Da-*

pidius granulosus, *Holoptychius*, *Diplopterus* and *Megalichthys*, by which the ganoid and placoid forms merge in one another.

That ganoid scales consist of variously modified osseous lamellæ, the result of successive additions made chiefly to the lower surface of each; but also, under particular circumstances, either to a part, or to the whole of the upper surface.

That these lamellæ have not been the result of any process of excretion, or depositions from a secreting surface, as supposed by M. Agassiz, but that they have been formed by the calcification of the lower laminæ of an investing vascular periosteum; and that consequently the phenomena attending the structure and growth of these ganoid scales contribute in a material degree to establish the correctness of the views recently promulgated by Professor Sharpey respecting the growth and development of human bone; the gradual formation of Haversian canals, being traced with great ease from the simple laminæ seen in the scales of *Lepidosteus*, *Lepidotus*, &c., through *Aspidorhynchus*, *Acipenser*, *Holoptychius*, &c. to their high degree of development in *Megalichthys*.

That the study of the microscopic structure of the dermal appendages of fish may, when carried on with due caution, be made a valuable auxiliary, both in distinguishing between allied species, and in establishing the existence of important affinities, even when applied to otherwise insignificant fragments; but that it is capable of being overstrained, and of leading to erroneous conclusions, if any classifications are founded upon it irrespective of the other portions of the fish to which the scales belong, because of the unequal ratio in which the various parts of an organism may have been developed. Thus, whilst *Lepidosteus osseus* presents one of the simplest forms of ganoid scales, it has the concavo-convex vertebral articulations of the Ophidians; on the other hand, in many species, as in *Megalichthys* and *Holoptychius*, whilst the structure of each part of the exoskeleton is highly developed, the vertebræ appear to have the double concave articulation common amongst fish and enaliosaurs.

The author, in conclusion, acknowledges his obligations to Sir Philip M. de Grey Egerton, M.P., Dr. Mantell, Mr. Binney, Mr. J. E. Gray and Mr. Searles Wood, for their valuable co-operation in supplying many important specimens for examination.

“On the Mechanical Equivalent of Heat.” By J. P. Joule, Cor. Associate R. Acad. Sciences, Turin, &c. Communicated by M. Faraday, D.C.L., F.R.S., Foreign Memb. Acad. of Sciences, Paris, &c.

After passing in review the experimental researches of Rumford, Davy, Dulong, Faraday, and others who have successively discovered facts tending to prove that heat is not a substance, but a mode of force, the author mentions the papers he has already communicated to the Royal Society, and published in the Philosophical Magazine, in which he has endeavoured to show that in the production of *heat* by the expenditure of *force*, and *vice versâ*, in the production of *force* by the expenditure of *heat*, a constant relation always subsists between the two. This relation he denominates the “Mechanical

Equivalent of Heat," and the object of the present paper is to advance fresh proofs of its existence, and to give to it the numerical accuracy requisite to fit it as a starting-point for further inquiries.

In carrying out the above design, the author has determined the relation of *work done* to *heat produced* in the cases of the friction,—1st, of water; 2nd, of mercury; and 3rd, of cast iron.

In the experiments on the friction of the fluids, the liquid was contained in a covered cylindrical vessel of copper or iron, and the agitation was effected by vanes of brass or iron, fixed to a vertical axis revolving in the centre of the vessel, whilst fixed vanes prevented the liquid being whirled in the direction of rotation. In the experiments on the friction of solids, a disc of cast iron was rotated against another disc of cast iron pressed against it; the whole being immersed in a cast-iron vessel filled with mercury.

The *force expended* was measured by the descent of the weights employed in rotating the apparatus; and great care was taken to correct it for the friction of the axes of the pulleys employed, &c.

The *heat evolved* by the friction was measured by exact thermometers, and very laborious precautions were taken in order to eliminate the effects of radiation or conduction of heat to and from the surrounding atmosphere. The corrected thermometric effect was then reduced to a known capacity for heat, by means of extensive series of experiments made in order to ascertain the specific heat of the materials in which the thermometric effect was observed.

In this way the number of units of work, estimated in pounds one foot high, required to be done in order to develope one degree Fahr. in one pound of water taken at about 50°, was found to be as follows:—

772·692 from friction of water, a mean of 40 experiments.

774·083 from friction of mercury, a mean of 50 experiments.

774·987 from friction of cast iron, a mean of 20 experiments.

"On the Nitrogenous Principles of Vegetables as the sources of artificial Alkaloids." By John Stenhouse, F.R.S.

After observing that there are few departments in organic chemistry which during the last six or seven years have attracted more of the attention of experimenters than the artificial formation of the alkaloids, and attributing this fact to the interesting nature of this class of bodies both as regards their well-defined chemical properties and the important medical virtues which many of them possess, the author proceeds to state, that although attempts to form the natural alkaloids, such as quinine, cinchonine, &c., by artificial means have hitherto been unsuccessful, yet chemists have been enabled by various processes to procure artificially a considerable number of true alkaloids very analogous to those which occur in nature. The various methods by which this has been effected, such as by acting on essential oils with ammonia, by the destructive distillation of coal and animal substances, &c., are then enumerated and described.

It is also remarked as somewhat singular, that while so many other sources have been examined, no attempt should have been made to procure alkaloids from vegetable albumen, fibrine, caseine, &c., which

are so rich in nitrogen, and which occur in such abundance in many plants. What renders the neglect of these substances the more remarkable, is the consideration that coal has been one of the most productive sources of the alkaloids, yielding them, as it does, four other bases besides ammonia. Now as coal is universally admitted to be of vegetable origin, and to consist of the remains of a variety of extinct vegetables, the nitrogenous principles of which must be regarded as the sources of the bases which it yields, it seemed to the author not unreasonable to expect, that, by acting on the nitrogenous principles of recent vegetables, the same organic bases as those obtained from coal, or at any rate a series of analogous bases, would be obtained in still greater abundance; and it subsequently appeared that this latter expectation was not altogether without foundation.

From the difficulty of procuring vegetable albumen, fibrine, &c. in a state of even tolerable purity, those portions of plants (usually their seeds) were selected which contain those principles in the greatest abundance.

In the first instance, a quantity of *Phaseolus communis*, or common horse-bean, was destructively distilled in a cast-iron cylinder, and the products collected by means of a large condensing Liebig's apparatus. These products closely resembled those obtained from the distillation of bones and other animal matters, comprising among other substances acetic acid, empyreumatic oils, tar, a great deal of ammonia and several organic alkaloids. The crude product was supersaturated with muriatic acid, and the clear liquid decanted after the tar had subsided. The acid liquor was next passed through a cloth filter, which removed the greater portion of the resinous matter. The clear liquid was then poured into a capacious still, and supersaturated with carbonate of soda. When the liquid began to boil, much ammonia was disengaged, and a quantity of oily bases collected in the receiver. Their amount increased as the distillation proceeded. These bases were separated from the ammoniacal liquid by means of a pipette, and were purified by suitable processes which it is unnecessary to particularize. These bases, though they were found to vary very considerably in their boiling-points and some of their properties, were very similar in other respects. They were transparent colourless oils, which were all of them lighter than water, and refracted the light strongly. Their taste was hot, resembling that of oil of peppermint. They all exhibited strong alkaline reactions, and neutralized the acids perfectly, forming crystallizable salts. The most curious circumstance respecting them was, that they were apparently quite different from the series of bases obtained from either bones or coal, and contained no aniline.

One of these bases was isolated and subjected to analysis. It boiled between 150° and 155° C. Its formula was found to be $C_{10}H_6N$, which differs only by two equivalents of hydrogen from nicotine. The only obstacle which has hitherto prevented the separation and examination of each of these bases individually, has arisen from the difficulty of procuring them in sufficient quantity. Not that beans when distilled yield bases in so much smaller quan-

tities than bones and other animal substances ; but as both bones and coal are distilled on the largest scale for commercial purposes, their crude oils may be easily procured in any quantity, and from these their respective series of bases may be readily prepared. In regard to the bases from beans and other seeds, the case is quite different ; as the scientific chemist is compelled to distil these substances on purpose, an operation which cannot be conveniently conducted in a laboratory, as it requires an apparatus so large as to be almost upon a manufacturing scale.

Oil-cake.—As the *Phaseolus communis* was regarded as the type of the Leguminosæ, oil-cake, or the expressed seeds of *Linum usitatissimum*, was selected from that numerous class of plants in which the starch of the Gramineæ is replaced by oil. The products of its distillation were very similar to those from beans, containing however more ammonia and a somewhat smaller proportion of oily bases, which, though similar, appeared to differ from those of the preceding series. They were also equally devoid of aniline.

Wheat, *Triticum hybernum*, and subsequently peat from the neighbourhood of Glasgow, were also destructively distilled. Both of these substances, in addition to ammonia, yielded a series of oily bases, which also contained no aniline.

Distillation of wood.—The author proceeds to state, that through the kindness of an extensive pyroligneous acid manufacturer he was enabled to examine considerable quantities of the crude acid liquor obtained from the destructive distillation of beech, oak, and other hard woods. The stems and larger branches of trees are alone employed for this purpose. He found to his surprise that this acid liquor contained scarcely a trace of ammonia or of any other organic base, showing that the woody portions of the limbs and stems of trees are nearly devoid of nitrogenous matter, in which respect they differ extremely from peat, which in general contains two per cent. of nitrogen ; and he considers this circumstance as perhaps calculated to throw some light upon the origin of the coal-beds, which some geologists believe to have been formed from the submersion of forests and the floating of uprooted timber into estuaries and lakes, while others contend that they have been produced by the submersion of beds of peat. Irrespective therefore of other considerations, the author urges in favour of the latter opinion, that wood is not capable of furnishing the amount of nitrogen we find existing in coal, while peat contains rather more than double the quantity required. The expectation of procuring aniline, picoline, &c., the coal series of bases, from the distillation of peat, was disappointed ; a result only to be accounted for on the hypothesis, that the different genera of plants, when destructively distilled, yield different series of organic bases.

From the facts which have previously been stated, the author considers himself warranted in concluding that when ammonia is produced by the destructive distillation of either animal or vegetable substances, it is always accompanied with the formation of organic bases. Now as ammonia is known to be procurable from these substances by other methods than destructive distillation, it seemed

highly probable that on these occasions organic bases would also be produced. Beans, oil-cake and flesh, were therefore successively boiled in a distilling apparatus with strong alkaline lyes. In every instance, in addition to ammonia, a series of organic bases was also produced. Similar results were also obtained when the above-mentioned substances were digested in strong sulphuric acid, the acid solution supersaturated with an alkali and subjected to distillation. The ammoniacal liquor which passed into the receiver was found invariably to contain organic bases.

Bases by putrefaction.—As putrefaction is almost the only other means by which ammonia is readily procurable in quantity from vegetable and animal substances, the effects of this process were also examined in the first instance in the case of guano. An aqueous solution of Peruvian guano was saturated with carbonate of soda and distilled. In addition to much ammonia, a quantity of basic oils was also obtained. Subsequent to this experiment the effects of putrefaction on a quantity of horse-flesh were also examined, when a considerable amount of oily bases was found to have been generated.

From the facts which have now been enumerated, the author concludes "*that whenever ammonia is generated in large quantity from complex animal or vegetable substances, it is invariably accompanied by the formation of a larger or a smaller amount of volatile organic bases.*" If therefore researches similar to the present are actively prosecuted, and if the seeds and leaves of the various genera of plants are subjected to these or analogous processes, it seems not unreasonable to expect that the number of the organic alkaloids will ere long be considerably increased.

"On the Development and Varieties of the Great Anterior Veins in Man and Mammalia." By John Marshall, Esq. Communicated by Professor Sharpey, F.R.S.

The object of this paper is to state the result of observations on the metamorphosis of the great anterior veins in Man and Mammalia, and on the relations existing between the primitive and final condition of these vessels, in different cases, both in their normal arrangement in animals, and their abnormal condition in the human subject.

From an examination of the form and structure of the sinus of the great coronary vein, and of the arrangement of its branches and valves in Man and some of the Mammalia, and from a comparison of those parts with the terminations of the great coronary and other posterior cardiac veins in the other Mammalia, the *coronary sinus* in Man and one set of Mammals, as the Dog, Cat, and Seal, is shown to be *analogous to the lower part of the left vena cava anterior* found in another set, represented by the Elephant, Rabbit and Hedgehog, and to *the lower part of the left vena azygos*, found in a third set, as exemplified in the Sheep, Ox and Pig. The great coronary vein, therefore, is shown always to end in a similar way, viz. in a larger muscular venous channel, which, in all cases, ends in the right auricle of the heart, by a wide orifice situated in an exactly corresponding part of that cavity.

The author remarks that a similar view has recently been published by Bardsleben; but his own observations were completed, and his deductions arrived at, quite independently.

Reflecting on the above-mentioned analogies and on the known method of development of the great anterior veins in *all* the Vertebrata, as pointed out by Rathké, from four primitive lateral venous trunks, viz. two anterior or jugular, and two posterior or *cardinal* veins, the coronary sinus is demonstrated to be the lower persistent portion of the left anterior primitive venous trunk, next to the heart. By Rathké, however, the whole of this left primitive trunk, from the neck down to the heart, is supposed to become occluded and then entirely to disappear in Man, and in such animals as are similarly formed in respect to these great veins; but the author finds that not only does its lower part persist in a previous condition as the coronary sinus, but that other remnants or vestiges of this primitive venous channel are to be found throughout life in Man, and in those animals in which the great anterior veins undergo a like metamorphosis.

The inquiry thus opened is then systematically pursued, first, by tracing the details of the metamorphosis of the great anterior veins in the embryos of the Sheep and Guinea Pig, and in the human *fœtus*; secondly, by a comparison of the adult condition of these great veins in the entire class of Mammalia; and thirdly, by an examination of the occasional varieties of the same vessels met with in the human subject.

1. *Of the development of the great anterior veins.*—After describing at length the metamorphosis of these vessels, the author proceeds to give an account of the remnants of the left anterior primitive vein in the adult.

These are indicated by the following parts, traced from the summit of the left thoracic cavity down to the back of the heart. *Outside the pericardium certain fine bands of fibrous tissue*, which descend beneath the pleura, from the trunk of the left superior intercostal vein to the front of the root of the left lung; and inside the pericardium, *a fold of the serous membrane* which passes down from the left pulmonary artery to the subjacent pulmonary vein,—*certain opaque lines or streaks* upon the side and back of the left auricle,—*a small oblique auricular vein* which is continued from those streaks down to the coronary sinus,—and lastly, the *coronary sinus* itself. The fold of the pericardium, which hitherto has escaped observation, is particularly described. It is named by the author the *vestigial fold of the pericardium*, or, from its having contained the canal of Cuvier in the embryo, the *Cuvierian fold*.

2. Under the second head, a *comparison* is instituted between the *great anterior veins of Man and the Mammalia* generally.

Having remarked that, as high up in the vertebrate scale as Birds, no fundamental alteration occurs from the primitive condition of two anterior and two posterior independent lateral venous trunks, the author remarks that in *all* Mammalia one *characteristic* change is met with, viz. the formation of a transverse branch across the root of the neck.

The right anterior primitive vein in all cases persists as the right or ordinary vena cava superior; but the left vein either remains unoccluded, and returns the blood from the left side of the head and neck, from the left upper limb, the left side of the thorax, and from the substance of the heart; or, owing to a partial occlusion, returns only the blood from the left side of the thorax and from the substance of the heart; or, owing to still further occlusion, from the substance of the heart alone. Hence three principal groups arise.

a. In the first group a right and a left superior vena cava exist, connected by a cross branch at the root of the neck, as in the Monotremata, Marsupialia, the Elephant, most Rodentia, the Hedgehog and the Bat.

b. In another group a right superior cava and a left vena azygos exist, as in the Sheep, Goat, Ox, Pig, Horse, Mole and Guinea Pig.

c. In the third group there is found, besides the right vena cava superior, only a left cardiac venous trunk or coronary sinus, together with the vestiges already described, as in the Cetacea, Carnivora and Quadrumana, as well as in Man.

In each of these groups subordinate varieties are shown and classified.

3. *The almost numberless varieties of the great anterior veins in the human subject* are then arranged on principles similar to those adopted in regard to the different conditions found among Mammalia; but the groups are arranged in the inverse order, and the usual condition of the veins in Man is included as a necessary element in the series.

In one large *class* of cases, comprehending *three groups* similar to those of the different Mammalia already defined, the cross branch in the neck is always present.

a. In the first group there is a right vena cava superior, and a left cardiac venous trunk or coronary sinus. This is the ordinary condition. Further subdivisions arise, depending on peculiarities of the vena cava itself, which are rare; of the azygos system, which are exceedingly numerous; and of the coronary vein and sinus, which are again uncommon. Transposition occasionally produces a further modification, in which the superior cava is found on the left side; whilst the coronary sinus, the oblique vein and the vestigial fold of the pericardium, exist on the right.

b. In another group there might exist a right vena cava superior and a left vena azygos, as in the Sheep; but no example of this possible variety has yet been met with in the human subject.

c. In the third group a right and a left superior cava coexist, as in the Elephant, constituting what is termed a double vena cava superior. Thirty examples of this condition are adduced, of which eleven only have occurred in adult and otherwise perfect hearts. One of these was met with by the author, and is specially described.

Lastly, a separate or second *class* consists of those cases in which the cross branch is wanting, and which are, accordingly, destitute of the characteristic mammalian type, and present, as in Birds, the persistent condition of four independent lateral venous trunks.

The paper is illustrated by original drawings, of the development

of the veins in the Sheep and in Man, of the vestiges of the left primitive vein ordinarily found in the adult human subject, and of the fresh example of double vena cava superior in Man met with by the author.

“A Mathematical Theory of Magnetism.” By William Thomson, M.A., F.R.S.E., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow.

The theory of magnetism was first mathematically treated in a complete form by Poisson. Brief sketches of his theory, with some simplifications, have been given by Green and Murphy in their works on Electricity and Magnetism. In all these writings a hypothesis of two magnetic fluids has been adopted, and strictly adhered to throughout. No physical evidence can be adduced in support of such a hypothesis; but on the contrary, recent discoveries, especially in electro-magnetism, render it extremely improbable. Hence it is of importance that all reasoning with reference to magnetism should be conducted without assuming the existence of those hypothetical fluids.

The writer of the present paper endeavours to show that a complete mathematical theory of magnetism may be established upon the sole foundation of facts generally known, and Coulomb's special experimental researches. The positive parts of this theory agree with those of Poisson's mathematical theory, and consequently the elementary mathematical formulæ coincide with those which have been previously given by Poisson.

The paper at present laid before the Royal Society is restricted to the elements of the mathematical theory, exclusively of those parts in which the phenomena of magnetic induction are considered.

The author hopes to have the honour of laying before the Society a continuation, containing some original mathematical investigations on magnetic distributions, and a theory of induction, in ferro-magnetic or diamagnetic substances.

“On the Structure of the Dental Tissues of Marsupial Animals, and more especially of the Enamel.” By John Tomes, Esq. Communicated by Dr. Grant, F.R.S.

The author of the communication bearing the above title, after examining microscopically the teeth of many marsupial animals taken from the majority of the families that make up the order Marsupialia, finds that they possess a structural character by which they may be distinguished from other mammalian teeth, subject only to one or two exceptions; in which exceptions, however, the teeth are small, and may readily be distinguished from marsupial by their external character. They are the teeth of the *Hyrax Capensis*, the British Shrews, and the molar teeth of the Jerboa.

The author states, that so far as he has had opportunities of examination, the teeth of the various species may also be distinguished, the one from the other. He points out, for instance, that, on comparison, the teeth of *Dasyurus ursinus* may be distinguished from the *D. macrourus*.

The peculiar characteristic of marsupial teeth exists in the con-

tinuation of the dentinal tubes into the enamel; so far as the author has investigated them, he finds but one exception, and that in the Wombat,—the representative of the rodents in the marsupial order. This creature, he finds, has teeth that are nearly allied in structure as well as external form to the teeth of rodents, and more especially to the Hare and Rabbit.

The author states, that he has observed that the dentinal tubes in the human and other teeth are sometimes continued for a short distance into the enamel. This he considers a rudimentary condition which is fully developed in the marsupial teeth. The author observes that the dentinal and enamel pulp become firmly united to each other previous to the commencement of calcification in either, and that it is highly probable that the linear columns of the two pulps are joined end to end, and that the columns of the enamel pulp so joined become developed into tubes instead of into solid enamel fibres. He considers this the more probable, as he has observed that the enamel fibres in an early stage of development are partially tubular in the teeth of several animals whose enamel fibres are ultimately solid.

The teeth described and figured are those of the—

Macropus giganteus.

Hypsiprymnus penicillatus.

— minor.

Phalangista vulpina.

Wombat.

Petaurus taguanoides.

Petaurus sciureus.

Dasyurus macrourus.

— ursinus.

Thylacinus cynocephalus.

Didelphis virginiana.

The author considers that the facts stated in his paper justify two conclusions of a general character. First, that the existence of prolonged and fully-developed tubes in the enamel, continuous with those of the subjacent dentine, is common to the great majority, if not all, of the marsupial animals, excepting the Wombat. And, secondly, that the enamel and dentine are so closely related, that they should be regarded as modifications of each other, rather than as tissues of a wholly different nature.

“On the Motion of Gases.”—Part II. By Thomas Graham, F.R.S. &c.

The experiments of the former paper by the author on the same subject, afforded grounds for assuming the existence of a relation in the transpirability of different gases, that is, their passage through capillary tubes of an equally simple nature as that which is recognized among the specific gravities of gases, or even as the still more simple ratios of their combining volumes. Compared with solids and liquids, matter in the form of gas is susceptible of small variation in physical properties, and exhibits only a few grand features. These differences of property, which are preserved amidst the prevailing uniformity of gases, may well be supposed to be among the most deep-seated and fundamental in their nature with which matter is endowed. Under such impressions he has devoted an unusual amount of time and attention to the determination of this class of

numerical constants. As the results, too, were entirely novel, and wholly unprovided for in the received view of the gaseous constitution, of which indeed they prove the incompleteness, it was the more necessary to verify every fact with the greatest care.

The most general and simple of the results is, that the transpiration velocity of hydrogen gas is exactly double that of nitrogen gas. These gases, it will be remembered, have a less simple relation in density, namely 1 to 14. This was the conclusion respecting the transpiration of these gases of his former paper, and he has obtained since much new evidence in its favour. The transpirability of carbonic oxide, like the specific gravity of that gas, appears also to be identical with that of nitrogen.

The result which may be placed next in point of accuracy and importance is, that the transpiration velocity of oxygen is related to that of nitrogen in the inverse ratio of the densities of these gases, that is, as 14 to 16. In equal times it is not equal volumes but equal weights of these two gases that are transpired, the more heavy gas being more slowly transpired in proportion to its greater density. Mixtures of oxygen and nitrogen have the mean velocity of these two gases, and hence the time of air is also found to be proportional to its density when compared with the time of oxygen.

The relation between nitrogen and oxygen is equally precise as that between nitrogen and hydrogen. The densities calculated from the atomic weights of oxygen and nitrogen, namely, 16 and 14, being 1 for oxygen, 0.9010 for air and 0.8750 for nitrogen, the observed times of transpiration of equal volumes of the same gases are for oxygen 1, air 0.8970 to 0.9010, and for nitrogen to 0.8708. The result for carbonic acid, which is perhaps next in interest, appears at first anomalous. It is, that the transpiration time of this gas is inversely proportional to its density: when compared with oxygen or 0.7272, the time of oxygen being 1, their velocities will of course be directly as their densities. It is to be remembered, however, that carbonic acid is a compound gas, containing an equal volume of oxygen. The second constituent, carbon, which increases the weight of the gas, appears to give additional velocity to the oxygen in the same manner and to the same extent as increased density from pressure or from cold increases the transpiration velocity of pure oxygen itself. A result of this kind shows at once the important chemical bearing of gaseous transpirability, and claims for it a place with the doctrines of gaseous densities and combining volumes. The circumstance that the transpiration time of hydrogen is one-half of that of nitrogen, indicates that the relations of transpirability are even more simple in their expression than the relations of density among gases. In support of the same assertion may be adduced the additional fact, that binoxide of nitrogen, although differing in density, has the same transpiration time as nitrogen. Protoxide of nitrogen and carbonic acid have one transpiration time; so have nitrogen and carbonic oxide, as each pair has a common density.

The transpiration of twenty other gases and vapours is experimentally determined, and shown to be uniform, like the preceding

gases, with tube resistances varying in amount from 1 to 1000. This list includes protocarburetted hydrogen, olefiant gas, ammonia, cyanogen, hydrocyanic acid, hydrosulphuric acid, bisulphide of carbon, sulphurous acid, sulphuric acid, chlorine, bromine, hydrochloric acid, ether, methylic ether, chloride of methyl, coal-gas and the vapours of water, alcohol and coal-tar naphtha.

The principal results respecting the transpiration of these vapours, and on the influence which pressure and temperature have upon the transpiration of a gas, are summed up as follows:—

The velocity of protocarburetted hydrogen is 0·8, that of hydrogen being 1.

The velocity of chlorine appears to be $1\frac{1}{2}$ that of oxygen; of bromine vapour and sulphuric acid vapour the same as that of oxygen.

Ether vapour appears to have the same velocity as hydrogen gas; their densities are as 37 to 1.

Olefiant gas, ammonia and cyanogen appear to have equal or nearly equal velocities, which approach closely to double the velocity of oxygen.

Hydrosulphuric acid gas and bisulphide of carbon vapour appear to have equal or nearly equal velocities.

The compounds of methyl appear to have a less velocity than the corresponding compounds of ethyl, but to be connected by a certain constant relation.

The resistance of a capillary tube of uniform bore to the passage of any gas is directly proportional to the length of the tube.

The velocity of passage of equal volumes of air of the same temperature, but of different densities or elasticities, is directly proportional to the density. The denser the air, the more rapidly does it pass under a constant propulsive pressure.

Rarefaction by heat has a similar and precisely equal effect in diminishing the velocity of the transpiration of equal volumes of air, as the loss of density and elasticity by diminished pressure has.

A greater resistance in the capillary is required to bring out the law of densities, than appears necessary for the two preceding results; and a resistance still further increased, and the highest of all, to bring out the law of temperatures.

Finally, transpiration is generally promoted by density, and equally whether the increased density is due to compression, to cold, or to the addition of an element in combination, as the velocity of oxygen is increased, by combining it with carbon without change of volume, in carbonic acid gas.

It did not enter into the plan of the author to investigate the passage of gases through tubes of great diameter, and to solve pneumatic problems of actual occurrence, such as those offered in the distribution of coal-gas by pipes. But he states that the results must be similar, with truly elastic gases such as air and carburetted hydrogen, whether the tubes are capillary or several inches in diameter, provided the length of the tube is not less than 4000 times its diameter, as in the long glass capillaries of his experiments. The small propulsive pressure applied to coal-gas is also favourable to transpi-

ration, as well as the great length of the mains; and he therefore would expect the distribution of coal-gas in cities to exemplify approximately the laws of gaseous transpiration. The velocity of coal-gas should be 1.575, that of air being 1, under the same pressure. And with a constant propulsive pressure in the gasometer, the flow of gas should increase in volume with a rise of the barometer or with a fall in temperature, directly in proportion to the increase of its density from either of these causes.

These laws, it will be observed, are entirely different from those which direct the passage of gases through an aperture in a thin plate, or their flow into a vacuum as it is usually said, and could not be deduced, like the latter, from our speculative ideas respecting the elastic fluids.

“On the Automatic Registration of Magnetometers and Meteorological Instruments by Photography.”—No. III. By Charles Brooke, M.B., F.R.S.

The author describes the construction of an apparatus for registering the variation of the thermometer and psychrometer on one sheet of paper. As in the apparatus for registering the vertical force magnetometer, described in a former paper, the photographic paper is placed between two concentric cylinders, placed with the axis vertical, and carried round on a revolving plate or turn-table by the hour-hand of a time-piece, which makes half a revolution in twenty-four hours; thus each half of the paper presents a record of the variation of one instrument during twenty-four hours. The scales of the instruments are continuously impressed on the paper by placing fine wires opposite each degree across the aperture through which the light falls on the stem; the light transmitted by the empty bore is intercepted by these wires, and the darkened portion of the paper is marked by a series of parallel pale lines corresponding to each degree: thus the distortion of the scale arising from the varying direction of the pencils of light is corrected. Every tenth degree is marked by a coarser wire, and therefore a broader line, as also the points 32° , 54° , 76° , 98° ; one at least of these points will occur on each register, and the position of the extra broad line serves to identify the part of the scale to which the register relates.

An alteration in the mode of adjusting the wick of the camphine lamps described in a former paper is mentioned, by which the chance of smoking is considerably diminished; likewise the successful application of naphthalised gas, and of an oil-lamp, to photographic registration.

The paper concludes with the description of a new method of determining the scale and temperature coefficients of the force magnetometers, by which a greater degree of accuracy is presumed to be attained than by the methods ordinarily employed. Two magnets designed for self-registering instruments for the observatories at Cambridge and Toronto, having been submitted to this method, gave consistent results which indicate the law of the temperature coefficient to be sensibly different from that which has hitherto been assumed.

LXV. *Intelligence and Miscellaneous Articles.*

ON THE PASSAGE OF HYDROGEN GAS THROUGH SOLID BODIES.

BY M. LOUYET.

M. LOUYET has made a curious observation connected with the history of hydrogen: he found that when a horizontal current of this gas, emitted from a capillary orifice, was directed upon a sheet of paper held vertically and perpendicularly to the gaseous current, the fluid passes through the paper without being sifted, as might be expected, but retaining the form of a current, and so perfectly that it may be inflamed behind the paper, absolutely as if it did not exist. Spongy platina placed behind the paper became incandescent, and it is to be observed that the pressure under which this phenomenon is produced does not exceed that of 40 to 48 inches of water.

M. Louyet has also stated that hydrogen gas passes in the same manner through gold, silver and tin leaf, even double, and also through thin membranes of gutta percha, such as are obtained by evaporating a solution of this substance in chloroform.

Lastly, the author has observed that the same gas does not sensibly pass through pellicles of glass obtained by strongly blowing a bulb at the end of a tube, however thin they may be.—*Ann. de Ch. et de Phys.*, Septembre 1849.

QUALITATIVE AND QUANTITATIVE DETERMINATION OF PHOSPHORIC ACID. BY M. LECONTE.

The importance of phosphoric acid in vegetable and animal physiology is well known, and, in this point of view, the utility of detecting the presence and determining the quantity of this acid in food, manures, &c., has been duly appreciated, particularly in those manures intended to fertilize the soils in which wheat is grown.

For twelve months several chemists have been occupied with this question, which appears a proof both of the interest and difficulty attached to solving it. M. Raewsky's process by acetate of peroxide of iron, and that of M. Cotterau by nitrate of silver, have been described. M. Leconte states that he has found the soluble salts of uranium to be the most certain for detecting and determining the quantity of phosphoric acid, on account of the absolute insolubility of the phosphate of uranium, and the facility with which this salt precipitates notwithstanding the presence of other saline substances, acids, &c.

The quantitative determination of phosphoric acid in soluble phosphates is very simple. A solution of nitrate of uranium is prepared, of which every cubic centimetre precipitates 0 gr. 001 of phosphoric acid; a known weight of the phosphate to be analysed is taken and dissolved in a known bulk of distilled water, taking care to neutralize it; fifty cubic centimetres of this liquor are boiled in a flask, and, by the aid of a graduated tube, nitrate of uranium is added to it, till the liquor standing over the precipitate becomes limpid. It must be boiled for a second after each addition of the test solution.—*Journ. de Ch. Méd.*, Novembre 1849.

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